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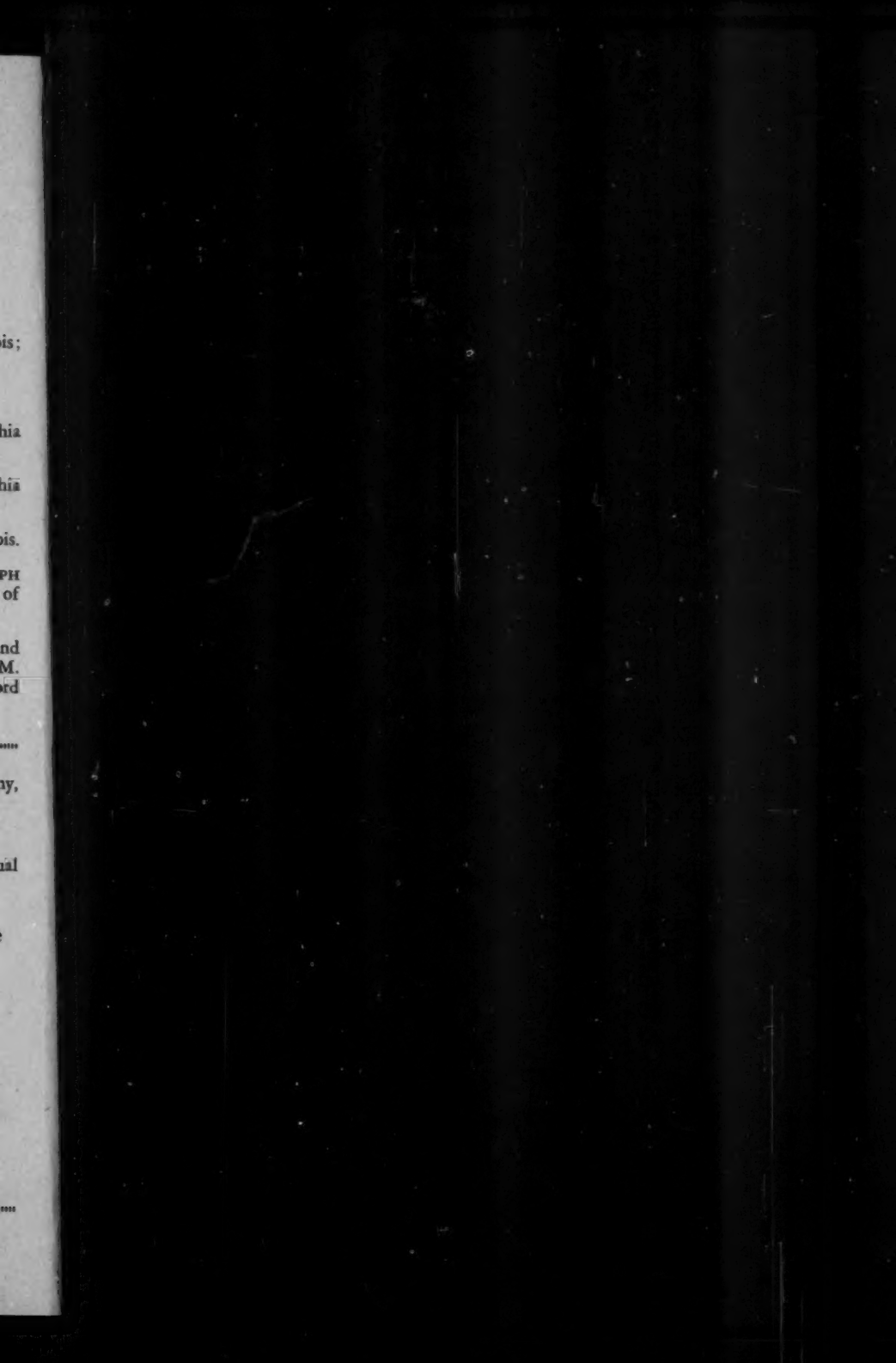

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ORIGINS OF AMERICAN INDIAN AGRICULTURE*

By GEORGE F. CARTER

INTRODUCTION

THE question of the origin of agriculture is of extreme interest for the insight that it gives us into human cultural processes, into the location of centers of early economic and intellectual advance, and the diffusion of influences as measured by the spread of useful plants. The recognition of the origins and the historic, nonenvironmental limitations on distribution of useful plants which arises from such study has many practical applications today. Attention in this paper will be focused therefore on the places of origin of American agricultures, and on the routes of diffusion of these agricultures.

This is largely a speculative synthesis of our knowledge at present. It should serve to call the attention of people in several allied fields to the work that is being done and to the very interesting possibilities being opened up for combined botanical, geographical, and anthropological research. Some will feel that it is too early to attempt a synthesis. Realizing the difficulties and dangers, it nevertheless seems worth while because of the impetus the whole field should gain from an attempt at putting the disparate pieces together. At present the pieces cannot be made to fit without much straining, and even then the structure is more reminiscent of a Swiss cheese than a brick house. There remain, however, a certain fit and coherence that are impressive. Future work will tighten up the gaps, and improve and correct this structure. If the building of this structure engenders work aimed at such correction and improvement, it is felt that this preliminary synthesis will have been worth while.

For a thorough discussion of the origins of American agriculture 75 to 100 separate plants would have to be considered. Neither space nor my knowledge permits such a consideration here. Hence I will concentrate on a review of the evidence from three species only. These will be corn, beans, and squash, the triumvirate that forms the basis of much of American Indian agriculture. They are wholly American in origin, and represent some of the greatest triumphs of the Indian in plant breeding, a field in which he excelled. But most importantly for our purposes, in the history of their origin, domestication, and diffusion

* Paper given before the Anthropological Society of Washington.

throughout the Americas lie some of the most important keys to the culture history of America. It is obvious that to trace these diffusions one must find the starting points for these plants.

Two methods are used in determining the original center, or centers, of domestication of plants. The most obvious is to find the wild ancestor of the present domestic plant. This is seldom possible. Our present domestic plants, both in the Old World and in the New World, have diverged so far from their ancestral forms that it is unusual for us to be able to point definitely to a wild ancestor. To the botanist this implies that great periods of time are involved. Vavilov,[1] the great Russian plant geographer and geneticist, has stated that the usual allowance of periods of 5,000 to 10,000 years is but a fraction of the time involved. Ames, of Harvard, has recently vigorously seconded this belief.[2]

The second method of determining the center of origin of a plant is to determine the center of varietal diversity. The Russian plant geographers have developed this system. They have found that there are areas where the range of variation within a given plant, domestic or wild, is very great. As one goes outward from this center one finds less and less variation. For example, in the American common beans there are numbers of flower types. All of these are found in Guatemala. As one goes north or south from Guatemala one finds fewer and fewer of these flower types. This is paralleled in other plant characters. The conclusion is that Guatemala is the home of the common bean. Occasionally this center of variability coincides with the distribution of related wild forms of the plant which probably include the ancestral form. E.g., there are wild beans in Guatemala. However, we are not yet able to point to any one of these beans as the probable ancestor of the common bean.

I plan to review our current knowledge of the origin and diffusion of the three best known of the many plants domesticated in America. It will become obvious that we do not know much, even of these three, but it should be obvious also that in the future we are going to know a great deal and that what we will learn is going to be of extreme interest to the anthropologist, botanist, geographer and others.

INDIAN CORN—*ZEA MAYS*

Corn has been under study for a considerable period and has accumulated a literature that is staggering in both its scope and volume. I can deal with but a fraction of the problem. Recent work by Mangelsdorf, Reeves, Cutler, Anderson, Cameron, etc., has given us a great deal of new material to work with and new insights into the entire problem.[3] Although it is difficult for the nongenet-

[1] Vavilov, 1926, p. 244. (See bibliography at end of article.) [2] Ames, 1939.

[3] Some of the most important recent papers on maize for this discussion are the following: Longelly, 1938; Mangelsdorf and Reeves, 1939; Mangelsdorf and Cameron, 1942; Anderson and Cutler. 1942; Anderson, 1943a, 1943b.

icist to evaluate critically some of the more technical arguments, much of the evidence is intelligible to the nongeneticist, and some of the material lends itself to ethnological and archeological checking.

The greatest recent advance has been Mangelsdorf and Reeves' work on the origin of corn. Their theory is threefold, and Mangelsdorf has summarized it as follows: "1. That cultivated maize originated from a wild form of pod corn which was once, and perhaps still is, indigenous to the lowlands of South America; 2. That Euchlaene (teosinte), the closest relative of maize, is a recent product of the natural hybridization of Zea and Tripsacum which occurred after cultivated maize had been introduced by man into Central America; 3. That new types of maize originating directly or indirectly from this cross and exhibiting admixture with Tripsacum comprise the majority of Central and North American varieties."[4] Anderson has summarized the various theories on the origin of maize and pointed out that though Mangelsdorf and Reeves' theory may not be the final word, it gives at present a better fit with the known facts than any other theory.[5]

The support for these theories rests upon genetics, botany, plant morphology, archeology, and history. The genetic evidence is as follows. The chromosomes of Andean maize lack centers of pigmentation called knobs. North American, Central American, and northeastern South American maize, however, has a varying number of knobs on its chromosomes. Tripsacum, the grass closely related to maize, has knobs on its chromosomes. In botanical experiment Tripsacum has been crossed with maize, and among the resultant plants has been a teosinte-like plant. Teosinte backcrossed with maize gives rise to characteristic North American knobby-chromosomed maize. The conclusion is drawn that maize was originally knobless, that the knobs on the chromosomes of maize are derived from crossing with Tripsacum, and that the North American maize varieties differ from Andean maize in the possession of tripsacoid characters.

Some of the tripsacoid morphologic characters which differentiate North American maize from Andean maize are: woodier plant, cob, and stem; more regular and closely spaced rows; more regular seed shape; abundance of dent corn; stronger, more fibrous root system; less hairy leaves; longer and more cylindrical cobs. These traits have been shown to be correlated with high knob-number and hence to be probably derived from Tripsacum crosses.

Archeologically we know that the Andean, morphologically nontripsacoid maize, is quite old in Peru. It occurs for instance in the Nazca culture, variously dated from 1000 B.C. to the time of Christ. In the Basket Maker levels of our own Southwest there is maize that is much less tripsacoid than later types

[4] Mangelsdorf and Cameron, 1942.

[5] Anderson, 1943.

of corn. This makes it appear likely that Andean corn precedes tripsacoid corn, even to the extent that the spread of corn northward toward the United States was under way prior to the Tripsacum-maize cross.

We have further evidence of the lateness of corn in the Middle American area and in the United States in the presence within this zone of multiple starch foods. The Russians pointed out that this was probably evidence both for the plural origin of plant domestication and for the priority of the local competitive starch foods. Dr. Sauer elaborated this theme in discussing the origin of American agriculture and concluded that it was highly unlikely that any people who possessed so excellent a starch food as corn would undertake the long and arduous task of domesticating a competing starch food. He argued therefore, that Colombia's arracacha, West Mexico's salvia, Brazil's manioc, Peru's potatoes, etc., probably all precede maize in those localities.[6]

Botanically it has been argued that maize must originally have been considerably different from its present form. In its present form it is unable to spread its seeds or propagate itself. It has been argued that the original plant must have had some seed-disseminating device and that the seeds must have been protected by individual husks. There is a corn called pod corn, which approaches this condition in that it has protecting husks on each kernel. It appears occasionally in modern corns. Historically it is known to have been of some importance in the Paraguayan region at the time of European contact. The finding of another extraordinary type of corn with almost all the known dominant genes of maize and of low knob-number has been reported from this same area.[7] It has been argued on ecological grounds that it is unlikely that corn was a Peruvian highland or desert plant and that the likely place to seek its wild ancestor is in the foothills of eastern Bolivia or in the savanna-forest border beyond. Many lines of evidence converge, therefore, and all tend to point to South America and especially to the east Bolivian-Paraguayan area as the probable home of corn.

One of the useful things about corn is that it is divisible into races just as is mankind. One might note too that although this division presents just as many problems as in mankind, in corn one has the advantage that the material can be grown, hybridized, and studied in many ways that are not available to the physical anthropologist. Because of this possibility of racial study, one can foresee a future when we will be able to map the distribution of the races, trace their movements from region to region and, when sufficient archeological material is at hand, even trace these movements through time. We can expect eventually to have genetic and morphologic measures of the degree of relationships of the various races.

[6] Sauer, 1936.

[7] Mangelsdorf and Reeves, 1945. These authors also give other evidence for the possible east Bolivian home of maize.

Our knowledge is now quite spotty but some of the details are beginning to emerge. Anderson and Cutler have made the first attempt to consider maize from a "racial" standpoint.[8] They have pointed out that the usual classification of maize into flint, flour, dent, pop, and sweet is a useful device but that it is an artificial catalogue. The different categories are of different genetic value; e.g., flint and flour are single gene differences, while denting is the result of multiple genes. Denting also seems to reflect the presence of Tripsacum genetic material. Clear-cut cases of "races" of corn may actually have all types of kernel. In short, this classification is no better than classifying man by cephalic index alone.

A more meaningful classification can be built on multiple characters. Some characters have been found to be relatively free of environmental influences, and to be based apparently on important genetic relationships. Some of these are ear form, tassel characters, leaf type, number of leaves per plant, root type and kernel. Each of these has a considerable range of variation which can be scored to give comparative values. Thus leaves are hairy, slightly hairy or glabrous. Ears may have relatively large or small cobs and varying number of rows, the rows may vary from straight to very irregular, the ear may taper toward both butt and tip, may be expanded at the butt or taper strongly from the butt to the tip, etc. Tassels vary greatly and promise to be diagnostically important. Roots vary from fibrous to coarse. Kernels vary greatly not only in endosperm character (flint, flour, dent, etc.) but in shape, size, color, regularity, etc. These characters tend to occur in characteristic association. Thus just as we know of a dolichocephalic, tall, blond, straight-haired, high-nosed human race, so we also find a race of corn with strongly pubescent drooping leaves, poorly branched tassels, superficial and weak root-system, with ears that are strongly tapered from the butt to the tip, and with kernels that are predominantly white and that tend strongly to be dented. But just as in the "Nordic" race one finds blonds, brunets, and redheads, so in the Mexican Pyramidal race described above one finds both flint and flour type endosperms, as well as the predominant dent kernels.

Several races of corn are now recognized. We can speculate on their relationships, but much of it is just speculation at present. We are not yet clear as to which are major and which are minor races, but the future holds the promise that there will eventually be certainty concerning these relationships. The principal races now recognized are as follows.

Peruvian races of corn

Even though this is the oldest center of maize agriculture that has been studied archeologically and ethnologically, and although we have considerable

[8] Anderson and Cutler, 1942. For an example of racial study of corn in a limited area see: Carter and Anderson, 1945.

Peruvian maize in our American collections, it has as yet not been systematically studied in the sense that the Middle American maize has. Expectably there would be the greatest variety of maize in this ancient center, both because of its age and because of its great variation in environment. However, the results of the Tripsacum hybridization in Middle America has tended to overshadow the great variation that does occur in Peruvian maize.

There are several outstanding characters in the Peruvian maize. There is a tendency to form pyramidal ears. The kernels of the seeds are normally elongated and pointed toward the top. Rows are wide spaced, and the pointing of the grains increases this effect. Rows are frequently irregular, often so irregular as to be impossible of counting. Flour corns are very frequent. Cobs tend to be relatively small and the stems are small. The plant is not woody. The leaves are broad, the tassels are dark green and the knob number is very low. The ears are relatively small, at least never showing the great elongation that marks some of the North American maize. The seeds are loosely attached to the cob and hence easily shelled off.

There seem to be at least three races in this area. One primitive race is a small-cobbed corn with relatively long seeds, which are rounded, not pointed, at the top. This is found modernly in the Andean highlands and in Chile, and archeologically in the earlier cultures, e.g., early Nazca.[9] By its relation to Nazca corn it suggests that much development of corn has occurred since Nazca times, and thereby suggests considerable antiquity for Nazca. The general primitive character of this corn suggests that it may be a fair representation of the corn that was introduced into Peru from the east. It may also approach the type of corn that was carried up the Andes eventually to hybridize with Tripsacum and to set off the secondary development of maize. It is interesting, too, that ears of this maize bear a faint resemblance to the maize of the early Basket Makers (irregular rows, rounded kernels, irregular kernels, tapering to both butt and tip).

Probably allied to this primitive corn, and perhaps to be viewed as developed from it, are the races of big-grained flour corns of Peru. This corn has many of the characteristics enumerated above, plus greatly enlarged seeds. These seeds tend to be much elongated and pointed at the top. Cuzco flour corn, remarkable for its enormous seeds each often the size of a nickel, is perhaps but the extreme variant of this type.[10]

Probably quite separate from the two races described above is the minute-seeded, rice-like grained, tiny-eared maize grown particularly at very high al-

[9] Anderson, 1943b.

[10] It is worth mentioning that the Peruvians produced the large forms of almost all of the plants under discussion. They must have been by far the most skillful plant-breeders of America. They produced large-seeded vulgaris beans, giant lima beans, giant-seeded corn, and gigantic pumpkins.

titudes in Peru. Rowing is not evident in this race. The significance of the minute size of seeds and ears is not known to me. The absence of rowing is due to the development of both flowers in the spikelet. This is the identical condition seen in Country Gentlemen sweet corn.

It would seem that there are then at least these three races present in Peru. Each has a wide range of variation, and some of the variations may prove to be sub-races. The general picture is that of very long periods of variation, selection, and adaptation. Since in this area there was no hybridization to speed up the process it seems clear that the antiquity of maize growing is very great.

The Peruvian, or better Andean, flour corns continue up the highlands into Colombia. In northern Colombia flint corns of differing race appear. These are the dominant type through Central America to the plateau of Mexico. On the plateau of Mexico, centering about Mexico City, a third race appears. Because I believe this race to be related to the Andean races just discussed, I wish to take it up next.

Centering in the Mexican Federal District and concentrated on the Mexican plateau one finds a most marked race of corn called Mexican Pyramidal. The ears taper sharply from the butt to the tip. There is little husk compression at the base. Rows are regular to irregular and widely spaced. The kernels are usually dented and are usually white. In shape they are usually elongated and often pointed, sometimes sharply so. Floury endosperm is second only to dent in occurrence. Flint is less common. The plant is strongly pubescent and drooping leaved. The tassel is poorly branched and the root system is weak and coarse. The plants are very tolerant of low temperature and most economical in transpiration.

In many ways this corn resembles the big-grained Andean corn. It is low in chromosome number, has a weak root system, similar ear shape, and is strikingly similar in kernel shape. The appearance here of a secondary center of flour corn (the primary center is in Peru) also suggests this Andean relationship. In view of the probability that corn spread up the highlands of South American into the highlands of Central America, and the fact that in the Guatemalan highlands low knob number is correlated with pyramidal shape, it seems quite possible that this Mexican Pyramidal race is a little-modified representative of this Andean corn, which has persisted in the Mexican and Guatemalan highlands because of its adaptation to high altitude cold.

In Central America there is centered a large group of maize varieties that have a wide spread over the Americas. These have been called Tropical Flints. The term has been variously defined with considerable resulting confusion. There appears to be a dual division within the group.

Small-seeded Tropical Flints center in Chiapas and Guatemala. The ears have well-rounded butts and do not taper markedly either toward the butt or tip. Cobs are short to medium in length but proportionately broad. Rows are

very straight, and closely spaced. The kernels are predominantly flint, thin, relatively small, and very regular in size and shape. The plant is marked by a large, many-branched tassel. The race seems to have resulted from the Tripsacum-maize cross. The small seeds, straight rows, and relatively thick cob are all tripsacoid features. It is thought that after highland Andean corn crossed with Tripsacum to give rise to this flint corn which was well adapted to tropical lowland conditions, this corn spread through the lowlands of Middle America. As previously mentioned, this corn is dominant throughout Central America and into lowland Colombia. A second branch extends into the Caribbean Islands and into Venezuela, and expectably on into the Amazon. Reeves' recent work on knob numbers would tend to show this influence continuing with diminishing strength from Colombia through Venezuela, the Guianas, and Brazil to Uruguay.[11] Another branch reached the United States and is a dominant element in much of our Indian corn.

Big-grained Tropical Flint is closely allied to this race; e.g., its tassels are very similar. Its primary center of development is in Guatemala. The ears are long, straight, and slender. The cob tends to be large and to vary from much enlarged at the base to cylindrical. In the eastern United States and on the eastern margin of the Pueblo region where this maize appears it is characteristically enlarged at the base and has irregular rowing in the swollen area with very straight rows above. The swollen base is associated with large woody stems, a tripsacoid feature. The kernel is predominantly flint and in the United States is often yellow. This race, however, has a wide range of colors. The seeds are very large (100 seeds weigh 160-600 grams; Mexican dent, 100 seeds weigh 120-370 grams).

The origin of this race is not clear. It is clearly related to the small-seeded Tropical Flints. In the United States it is equally clearly later in arrival. It therefore seems likely that it is a later development and that it contains more Tripsacum genetic material.

The history of corn in the United States is obviously aside from the major development of corn. It is, however, of considerable interest to many of us. And by the sequence of arrival of corn types in the United States we can learn considerable about the sequence of happenings in the Middle American area. Eastern maize is characterized by low knob numbers.[12] Maize along the Canadian border reaches averages of less than one knob per plant. As one approaches the Gulf of Mexico this number increases to slightly over four. One can either interpret this as evidence of the introduction of pre-Tripsacum corn (and the evi-

[11] Reeves, 1944. Plant geographers have generally assumed that this was a good case of diffusion of cultivated plants from Middle America into the Caribbean Islands, and thence to Venezuela. Dr. Gordon R. Willey, however, has pointed out to me that the cultural evidence suggests much more strongly that the diffusion was probably from Middle America to Venezuela and thence back into the Antilles. This is a good example of the interdependence of all workers, and the failure of any one approach to supply all the answers. [12] Longelly, 1938.

dence from the Basket Makers would tend to bear this out), or point to the possibility that in selecting maize for high latitudes the Tripsacum characters were dropped out. However, one might also expect the opposite of this, i.e., that the Tripsacum characters for hardiness would be retained or even strengthened.

It is clear that we have elements of several races in the East. Along the Canadian border there was flint corn, of eight rows, and of course great cold and short-season tolerance. In the Dakotas this was a small-grained race; in New York and in New England it was a big-grained race. South of this lay a belt of flour corns. These are probably racially similar, but with at least the single gene difference for floury endosperm. South of this lay a broad belt of many-rowed dent corns. In racial terms this seems to read: small-grain Tropical Flint in the Dakotas, and big-grain Tropical Flint in New England. The later big-grain corn's appearance in New England may prove to be related to the relatively late movement of the Iroquois into that region. The dent corns of the United States seem to be a hybrid race composed of elements of the Tropical Flints crossed with the Mexican Pyramidal race. It is clear that the Mexican Pyramidal race is the dominant factor.

In the Southwest the picture is very complex, but perhaps only because we know more of the Southwest. Basket Maker II corn is moderately small eared but relatively large cobbed. The cob tapers to both butt and tip. The ear is irregular rowed, seeds are irregular in shape, flint in texture, and they often have a reddish brown coloration unknown to me outside of some Peruvian counterparts.[13] Racially it appears possible to consider it a little-modified Andean corn, with the modification being in the direction of Tropical Flint corn. It is followed in Basket Maker III by flour corn of somewhat similar type but with more regular rows. In early Pueblo times dent corns of the Mexican Pyramid race appear, but they rapidly disappeared leaving, however, traces of their characters in Pueblo corn. In late Pueblo times corn with giant ears and strongly enlarged butts appears. These last may be related to big-grain Tropical Flint. It is clear that the Southwest received a long series of corn importations; it is less clear as to where they came from.

The Gila-Colorado area to the south was held by a distinct race of corn. It is characteristically small eared, small cobbed, small stemmed, few rowed, irregular seeded, uniformly flour, uniformly white or light yellow, and relatively high knob-numbered for United States maize. It is a distinct and most uniform race. It goes back archeologically into the Hohokam culture. None of the Pueblo and Basket Maker corns can be derived from it, and hence not from the territory of northwest Mexico. It is therefore probable that Basket Maker and Pueblo corn came from the eastern United States.

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[13] However, Anderson has argued that the coloration was artificially produced. I have dissented elsewhere. Anderson and Blanchard, 1942; Carter, 1945.

The major flaw in this argument is that the Southwest corn today shows much higher numbers of chromosome knobs than does any Eastern corn. I suspect that part of the reason for this lies in nonanthropological collecting and in the use of a very small number of samples as a basis for the genetic work. Even then, however, it is clear that the Puebloan area has received a large dose of Tripsacum genetic material as measured by the presence of knobs. The number of knobs would suggest that the source was the Pima-Papago people. All other racial measures, however, indicate that this was not the primary source. Our present knowledge suggests a late (post 1000 A. D.) source of knobs from Pima-Papago corn for Hopi-Zuni, and a very late (post 1400?) source of knobs for the Eastern Pueblos, perhaps from the enormous-cobbed, woody-stemmed race of corn that invaded the Rio Grande and is now also widespread among the Navaho.

To summarize our knowledge of corn then: The present evidence indicates that the original home of the wild corn was in the lowlands of South America about the upper Paraguay river or in the eastern Bolivian area. Maize domesticated here was spread in relatively late times, agriculturally speaking, into the Andean region where it underwent a long series of developments, including selective breeding for size of kernels, etc. Flour corn may also have developed here and was apparently greatly prized for its ease of grinding.

Maize spread on up the Andes, being slowly adapted by other peoples, who in the main were already agricultural, and who simply fitted the new crop into the already existing agricultural complex. Not until maize had crossed the Panama region and was being grown in the Guatemala region did it undergo any great change. In the highlands of Central America or Southern Mexico, possibly in the highlands of Guatemala or quite possibly in several different places within the region, Andean maize crossed with Tripsacum. From this cross maize gained a whole group of new characters, some of which were to enable it to go into new territory.

Either prior to any contamination by Tripsacum, or before much such crossing had occurred, maize began spreading on northward eventually to enter the United States. Later, repeated waves of maize with increasing tripsacoid characters reached the United States. The exact routes are as yet little known. There are three possible routes; probably all three functioned: (1) up the west side of Mexico (Gila-Colorado race), (2) up the east side of Mexico, (3) across the Caribbean. By one or both of these latter, but not by the first, the Tropical Flints, the Mexican Dents (Mexican Pyramidal), and probably the Basket Maker Flints and the early Pueblo flour corns reached the United States.

BEANS—*PHASEOLUS*

Another group of plants important to American agriculture and useful in unraveling culture history is the American beans. The latter belong to the spe-

cies *Phaseolus* of which the Indians had domesticated four separate species. These are *vulgaris*, the common bean; *lunatus*, the lima bean; *coccineous* (*multiflorus*), the scarlet-runner bean; and *acutifolius*, the tepary bean.

Three of the beans (*vulgaris*, *lunatus*, and *coccineous*) show clear tropical traits. They tend to be perennial, to have long vegetative periods, to show short-day responses, to have long liana-like stems, to develop starchy roots, etc. All of these are old features in these plants which have been modified under cultivation. For example, they all have been shifted from perennial to annual form. Bush forms have been developed. The plants have been adapted to growth under the long-day conditions of the extratropics, and to tolerance of some degree of cold and drought.

Phaseolus vulgaris (common bean)[14]

The greatest diversity of *vulgaris* beans is to be found in Guatemala, Oaxaca, and Chiapas. This would tend to indicate that this is the home of the common bean. Wild beans are known in the area, but a clear example of a wild *vulgaris* has not been demonstrated.[15] This is, however, the usual condition, for the domesticated plants of the world have diverged so far from the ancestral forms that it is very rarely possible to point with any certainty to the ancestral form. This fact is of some interest in demonstrating both the very respectable antiquity of American agriculture and its possible comparability in age to the agriculture of the Old World.

In the Guatemalan area is to be found the full range of flower forms of *vulgaris*. As one goes outward from this center the flower types become less numerous. By using such botanical characters the Russians traced the spread of the common bean as follows. One branch went northward into Mexico. Here the greatest diversity of seed color and shape occurs in Central Mexico, and beans are adapted to all conditions from sea level to 10,000 feet elevation. The Russians found over a hundred varietal names in common use. In northern Mexico sharply distinct types of beans are found. Bush beans become increasingly common and are adapted to extremely short seasons. There is a complete absence of giant-seeded forms of beans. All of this is evidence of great antiquity of bean cultivation in Mexico. Presumably it was from this Mexican center that beans were spread into the United States.

Yucatan is only partly related to Mexico and Guatemala in bean types. It contains many endemic types of beans, especially of *lunatus*. However, we are not clear as to the meaning of this. It may reflect separate origins, or it may

[14] This discussion is based in part upon the following: Ivanov, 1937; Bukasov, 1931; Mackie, 1943.

[15] However, Dr. F. W. McBryde tells me that among the plants collected by him in Guatemala there was a likely candidate for a *vulgaris* ancestor.

reflect long selection and development of beans suited to that peculiar environment.

On the basis of flower type, Peru, Colombia, and Bolivia are related in bean types. Peru and Bolivia have a few ancient forms of beans, and botanically this is clearly a late and secondary center. It started with a limited group of beans and developed a great variety of color coats and some large beans. But the flower types indicate the varietal poverty of the material with which they started their bean growing. It is much to their credit as master plant-breeders that, starting with so limited a base, they accomplished so much.

Colombia differs somewhat from Peru in both plant type and flower. There is no evident direct tie to Mexico. E.g., there are no small-seeded forms in Colombia as are found in Mexico. It would appear then that starting from a Central American center common beans were diffused southward through Colombia to Peru and Bolivia and probably also to Chile and Argentina. Northward from the same center another line of beans was diffused into Mexico, and from there beans must have reached the United States.

Curiously, Venezuela connects with Mexico via Cuba and the Caribbean Islands. Or perhaps this actually means beans spread through northern, lowland Colombia to Venezuela and thence to the Antilles. Venezuelan beans are not closely related to Colombian beans. This distribution is paralleled in other crops also. It has been reported that corn of the Middle American mainland is related to Antillean corn and that the relationships extended thence into Venezuela and on through the Amazon lowland. This apparently closely parallels the bean distributions.

In the United States relatively little is known of the beans of the Eastern Indians. It is important that what beans remain in Indian hands on the reservations be speedily gathered up. From the little that I have observed, there was a very considerable variety, with both large and small types. Coloring varied but there generally were many colors, and many of the beans had glossy seed-coats. In the Southwest much more is known, and interesting divisions can be made on the basis of races of *vulgaris* beans. Thus the Hopi and Zuni form a group apart from the rest of the Southwest in the possession of large, glossy-coated beans. It is apparent that these beans are related to the Basket Maker beans. The Rio Grande Pueblos possess small dull-coated beans. These seem to be representatives of the original beans they possessed, but we need archeological data to verify this judgment. The presence in the late Basket Maker levels of large beans is particularly interesting in view of the absence of such beans in Northwest Mexico. It should also be noted that the Hopi-Zuni beans can be matched today (or could in Gilmore's time)[16] with beans from tribes along the Missouri river. Basket Maker and Early Pueblo beans, then, seem to have come from the Eastern United States.

[16] Gilmore, 1911, p. 97.

Phaseolus acutifolius, the tepary bean

The tepary bean is a distinct species. It has a very limited distribution both archeologically and modernly. It is one of the few domesticated plants for which we have a reasonably good ancestral form. Its climatic adaptations, cultural relations, and its related wild forms all coincide to indicate that it was domesticated in Northwest Mexico. It seems probable that if one were faced with the choice of adapting a *vulgaris* bean to heat, drought, and short season, or of taking under domestication a bean with a seed about the size of the head of a pin, one would adapt the *vulgaris* bean. That *vulgaris* could be so adapted is indicated by the very short-season bush beans characteristic of Northwest Mexico outside the tepary area. That the tepary had been domesticated and improved to equivalent worth of the *vulgaris* bean prior to the arrival of domestic *vulgaris* beans is indicated by the failure of *vulgaris* beans to invade the tepary area.

The tepary is most important among the Yuman and Pima-Papago peoples. It extends southward into Mexico among the Pimans but rapidly is replaced by the *vulgaris* bean south of the Piman people. There are outliers of tepary growing however as far south as Guatemala. Northward it is limited almost exclusively to the Hopi and Zuni. Archeologically, so far as known, the tepary is associated with the Hohokam, appears in the Puebloan area only late in pre-Columbian time and appears first on the southern border of Pueblo area. *Vulgaris* beans seem never to have been important, if grown at all, in the Gila-Colorado area in prehistoric times. Taken with the absence of giant-seeded *vulgaris* beans of Basket Maker type from Northwest Mexico this seems to indicate that the Basket Maker beans did not come from Northwest Mexico. Thus we are unable to derive either Basket Maker and Pueblo corn or beans from Northwest Mexico.

Phaseolus lunatus, the lima bean

The lima bean is rather puzzling. DeCandolle, the great Swiss plant geographer of the last century, thought that the lima had been domesticated in Brazil. Mackie has recently favored a Guatemalan origin.[17] The Russians also favored a Guatemalan origin but they placed the origin of all beans but the tepary in Guatemala. Mackie's claim of a wild lima from Guatemala is interesting, but it rests on a very extensive collection made in Guatemala by McBryde. Lacking similarly extensive collections from other likely areas, the evidence is at present a bit lopsided. Yucatan with its many wild and endemic forms of limas might be an equally likely home. But south Brazil and the Paraguay region have not been disposed of, nor has Venezuela. But all of these are possible homes of the lima bean. Mackie differentiates three major races of lima beans: the Caribbean, the Hopi, and the Peruvian. These are respective-

[17] Mackie, 1943.

ly plump and high in cyanide content, small in size, and large in size. Mackie attempts to reconstruct the spread of these races from a Guatemalan center. By his thinking, the Hopi branch spread up the west side of Mexico to the Hopi and thence eastward to the Appalachian region and the Atlantic coastal plain. Another branch spread through the Caribbean Islands and then into Venezuela and thence through the Amazon. The third branch he envisions as spreading down the Andes to Peru, where the giant-seed forms were developed. The parallel with the spread of the common bean and with corn is striking.

I have objected to the Hopi branch continuing on to the eastern United States on the grounds that lima beans are late to appear in the Pueblo area and are then limited, archeologically and recently, to the Hopi. Lima beans were certainly pre-Columbian in the eastern United States as is shown by Hariots' reference to them by an Indian name in 1585.[18] The Eastern Indian limas are of the so-called Hopi type, however, hence they could not have come from the Antilles. For this crop it becomes necessary to postulate an east Mexican corridor to the United States.

Mackie's theory could work as well in reverse. If the lima were domesticated in South America, it would spread up the Andes to Middle America and thence to the United States. Another branch of the lima could have travelled through the South American lowlands into Venezuela and thence into the Caribbean Islands. The fact is that we are really uncertain of the origin of the lima bean and need further data.

Concerning the scarlet-runner bean I have little to contribute. The Russians thought that it too may have been domesticated in Guatemala. In that area they found that it intergraded with *vulgaris* beans and they thought that *vulgaris* and *coccineous* were interbreeding to some extent. Elsewhere the two species are quite distinct. The scarlet runner appears in the Southwest ethnologically at Zuni. Whether this is a post-contact phenomenon or a result of Zuni contacts with the Salado and Hohokam, etc., is unknown. I have seen samples of *coccineous* beans collected in Michoacan, and one might assume they were fairly widespread in Mexico.

CUCURBITAS

The squashes and pumpkins are American domesticates belonging to the genus *Cucurbita*. There are three principal species recognized: *maxima*, *moschata*, and *pepo*. These species were sorted out in the mid-19th century and defined on the basis of mutual sterility. It has since been determined that this sterility is not absolute, but the separation is of such magnitude that the species probably never interbreed in nature.[19]

[18] Carter, G. F., "An Early American Description Probably Referring to *Phaseolus lunatus*," to appear in *Chronica Botanica*.

[19] The principal cucurbit literature is as follows: Bailey, 1929, 1930, 1932, 1943; Castetter and Erwin, 1929; Erwin, 1931, 1936a, 1936b, 1938; Erwin and Haber, 1929; McKay, 1932; Russell, 1924; Small, 1922 and 1930; Tapley, Enzie, and Van Eseltine, 1937; Vestal, 1931; Zhiteneva, 1929.

As with the beans, one might argue that if separately domesticated they should present distributional evidence of separate origin. In pre-Columbian times *maxima* was known only in South America south of Colombia, *moschata* centered in Middle America, and *pepo* was found only in North America and principally in the United States. Further, a wild form of *pepo* eminently suited to be the progenitor of the domestic *pepos* is known to exist in the southern United States. There is an almost equally satisfactory wild *maxima*-like cucurbit known in the northern Argentina-Uruguay-Paraguay area. Another supposed ancestral form, related to the *moschatas*, is reported to exist in Venezuela.

It will be noted that the reputed *maxima* ancestor is found in the same area in which corn is thought to have been domesticated. The *pepo* ancestor is found in an area where another independent agricultural beginning has been demonstrated. If the wild *moschata* is native to Venezuela, we can confidently expect that other domestic plants will also prove to have originated there, for our work in American plant domestication indicates that domestication always involved a complex of plants.

It is interesting that each of these species can be divided into two major varieties and that each has its own distribution. Thus *maxima* divides into common *maxima* and *maxima turbaniformis*. *Turbaniformis* seems to be Brazilian, and *maxima* to be Peruvian in distribution. *Moschata* has a brown-seeded form in northern South America and a white-seeded form in Central America and Mexico. *Pepo* has a multiplicity of seed forms. An eastern United States center of domestication of one form seems certain, which supplied the *pepo* pumpkin and squashes of the eastern Indians. A separate domestication of another form in Northwest Mexico seems indicated because of the inability to connect the two regions (eastern United States and western Mexico) and because the *pepos* of the two areas differ significantly in seed, fruit, flower, and leaf. This Mexican *pepo* was the aboriginal type of the Pima-Papago Indians. Again it is to be noted that each variety centers in an area where other plants were domesticated: sunflower, *iva*, *chenopodium*, etc. in the East; and tepary bean, Hopi cotton, and *salvia* in Northwest Mexico.

So we are led to admit not only that plant domestication is plural, but that different species of the same plant were sometimes independently domesticated, and finally we are led to the probability that varieties within a species were sometimes independently domesticated. It becomes increasingly clear that plant domestication occurred somewhat independently and roughly simultaneously in separate areas in the New World.

We know a few details of the diffusion of the *cucurbitas* in later times. The Mexican white-seeded *moschata* spread into the eastern United States, and appears also in the Basket Maker culture. By use of archeological evidence it can be shown that *moschata* appears first in the northern part of the Southwest and spread southward. It does not seem possible, therefore, that it came from western Mexico. This is made the more unlikely when one remembers that north-

west Mexico was anciently an area of *pepo* pumpkin growing. The conclusion seems unavoidable that the Basket Maker corn, beans, and squash came from Mexico via the eastern United States.

I have recently examined and reported on a group of seeds from pottery vessels collected in Peru by Max Uhle, and now preserved in the University of California anthropology Museum.[20] This limited collection included seeds from Nazca to contact cultures. The earliest seeds were *maxima*. In later times, *moschata* seeds appeared. Today there are a limited number of varieties of *moschatas* grown as far south as northwest Argentina. As in North America, *moschata* seems to have had the ability to spread rather widely. I do not know what the attraction of this plant could have been for the natives. It is perhaps worth noting that there were no *pepo* seeds. Earlier workers have also failed to report *pepo* seeds from Peru, and the supposed *pepo*-like pottery replicas are in reality replicas of *moschata* forms.

MAJOR LINES OF DIFFUSION

From these considerations of the origin and distribution of these three groups of plants it is possible to make out the major lines of agricultural diffusion in the Americas. One of the major routes was through the Andean and Central American highlands. It is clear that maize spread northward by this route and that *vulgaris* beans and *Cucurbita moschata* moved south along this same route. This seems then to have been the major route connecting the centers of high cultural development in America. The termini of the routes lay in Mexico in the north and led through Peru and Bolivia in the south. In the south continuations extended to Chile and to northwest Argentina. In the north continuations extended: (1) to the Gila-Colorado area of the United States and (2) into the Eastern United States. Judging by the tendency for crops to travel up the Andean highland one would expect one branch to have followed the Andes into Venezuela. We have little evidence that this was very active.

A Caribbean-Venezuelan route of considerable importance is indicated. A race of corn and a race of lima beans of the Central American mainland are clearly related to the Antillean and the Venezuelan corn and lima. If the cultural evidence is to be followed, the route of spread was from Central America by the lowlands to Venezuela, and thence one branch leading back into the Caribbean, and another leading south into the Amazon. Much of the argument regarding Colombian crops is based on Russian sources, and these writers apparently collected principally in highland Colombia. Lowland Colombia is likely to prove to be agronomically distinct. Study of the crop distributions in the Caribbean Islands, the Venezuelan, and in the Brazilian areas (all almost to-

[20] Carter, G. F., "Some Archeologic Cucurbit Seed from Peru." (To appear in an early issue of *Acta Americana*.)

tally unknown) should tell us a great deal more of the routes, times, and directions of these influences.

The routes of plant diffusion to the United States are of considerable interest. Pima-Papago corn, the tepary bean, "Hopi" cotton (*Gossypium punctatum hopi*, Lewton), and *Cucurbita pepo gilensis*,[21] a quite complete agricultural assemblage, obviously reached the Gila-Colorado area from the northwestern part of Mexico. This route almost undoubtedly followed the west coastal corridor defined by Sauer.[22] It leads into an area of extreme climatic difficulty. How inactive it was will be seen by comparison of the single race of corn associated with it to the numerous races of corn introduced into the eastern United States. None of the plants from the Gila-Colorado area can be shown to have been of any importance in the formative period of the Anasazi culture. However, the presence of these crops in the Hohokam culture would seem to indicate that these crops had reached the upper end of this corridor prior to the beginning of the Christian era.

If Pueblo agriculture cannot be derived from this area, one must construct another corridor or corridors to the United States. There is no evidence of the functioning of such a corridor on the western edge of the Mexican plateau. The little we know of the corn and beans of western upland Mexico indicates that Puebloan crops cannot be derived from this region. We know virtually nothing of the eastern side of the plateau except that it is climatically a good area for agriculture and leads with only the break of the Rio Grande valley from the broad-leaved forests of Mexico into the deciduous woodlands of Texas and thence into the eastern woodlands of the United States. Purely on geographic and agronomic grounds this is far and away the most feasible route from Mexico into the United States. In terms of crops one can point to the strong representation of the Mexican Pyramidal corn in the eastern United States as possible evidence of the functioning of this corridor. The positive relationship of the eastern lima beans to Guatemalan-Mexican lima beans but not to those of Antillean lima beans is also strong evidence of the likelihood of the reality of this east Mexican corridor.

Theoretically the Caribbean to Florida route should have functioned also. Evidence on it is conflicting. Longelly considers the chromosomal evidence from maize to support it. But the strong evidence from Mexican Pyramidal corn tends to disprove it, at least for this race of corn. The lima beans tend to deny it. We do not know the *vulgaris* evidence. The white-seeded *moschata* is

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[21] *Cucurbita pepo gilensis* is the name suggested for the squash domesticated in Northwest Mexico.

[22] Sauer, 1932. (Editor's note: The relatively very close linguistic relationship of Pima and Papago to Tepehuan and Tepecano, a thousand miles farther south, a resemblance comparable to that between Spanish and Italian, suggests a relatively very late separation of these groups, a very pertinent point in the present connection.)

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Mexican, but for all we know, it may also be Antillean. Culturally there is very little evidence for a Caribbean-mainland influence.

Times of crop diffusions:

We can speculate as to the time some corridor functioned to send corn, beans, and *moschata* to the dated Basket Maker agricultural beginnings, and back down the corridor to gain minimal dates for other areas. Basket Maker agriculture apparently begins with corn and *moschata* just after 1 A.D. Corn had to spread from the eastern United States and undergo strong climatic adaptation to reach the Basket Maker area. Five hundred years is probably little time to allow for this. Hence corn must have been introduced into the Eastern United States by 500 B.C. The history of the pre-corn agriculture of the East must lie back of this.

To trace this back further: maize must then have been introduced into Mexico long enough prior to 500 B.C. to allow for its spread on up into the Eastern United States, or, to pick another round number, another 500 years, or 1000 B.C. Maize must have taken a long period of time to spread up through the Andes, across the Central American lowland and into Guatemala and Mexico. If we allow 500 years for this we are back to 1500 B.C. for the start of maize up through the Andes. But if maize was domesticated in the lowlands to the east of Bolivia, then even longer periods must be added before maize even started its journey. I feel that these hypothetical dates are minimal, but the main points are that they are hypothetical, and that the farther we get away from the fixed Basket Maker dates, the less valid they are.

CENTERS OF DOMESTICATION

The major centers of plant domestication that emerge from a consideration of corn, beans and squash are then as follows: (1) the east Bolivian-Paraguayan area where *Zea mays* and *Cucurbita maxima* seem to have been domesticated; (2) the Middle American area where *Cucurbita moschata* was domesticated; (3) the Guatemalan area where *vulgaris* beans were probably domesticated, and where lima beans and *coccineous* beans were possibly domesticated; (4) the northwest Mexican area where tepary beans and a Mexican *Cucurbita pepo* were domesticated; (5) the southeastern United States where the commercial *Cucurbita pepos* were domesticated.

It must be understood that these are not the only plants domesticated in each of these areas, and that if the full list of American domesticated plants had been used more separate centers could be demonstrated. Enough has been done with these plants, however, to show the direction in which the work is going and the type of results expectable. Further work is urgently needed. The anthropologist can expect great aid from these studies in deciphering time and direction of cultural movement. In return he can contribute a great deal

through careful collecting of the crop plants of the peoples among whom he works. The utility of such plant materials in unraveling cultural problems is closely related to the skill with which it is collected. Obscure plants that never reach even local markets but which may be deeply embedded in local ritual may be of extreme usefulness in deciphering group relationships.[22] No one is as well equipped to do this careful seeking out of the little-known plants, and collecting and assessing the associated folk tradition as is the anthropologist.

But the anthropologist will have to rely on botanical and genetic aid in determining the relationships of the plants. In return, the materials the anthropologist collects should be of greatest aid to the agronomist, and of greatest interest to the botanist and geneticist. Some of the little-known races of domestic plants in the hands of the Indians are possessed of factors for disease, heat, and drought resistance. Their collection and study should repay us amply. It is to be hoped that botanists will interest themselves more in domestic plants. And one may hope that botanists and agronomists when collecting domestic plants will also gather data on their place in the people's culture and their probable antiquity in the area.

Such studies as this are also of great interest to a geographer. The origins and diffusion of domestic plants give him keys to understanding the regional differentiation of the earth's surface. Through such studies he gains an insight into the validity of "climatic" limitations. Attention to point of origin and route of diffusion frequently shows that domestic plant distributions are often culture-historical accidents, i. e., that their limits are cultural and not environmental.

The combined efforts of geography, botany, and anthropology applied to the study of American agricultural origins offer a most fruitful field for research productive of new insights for all. We need to know much more about corn, beans, and squash. But we need to know even more about manioc, potatoes, tomatoes, quinoa, etc. Neither the anthropologist nor the botanist nor the geographer can alone wrest the fullest knowledge from the domestic plants of America. But by working together, the riddles of the origins of American agriculture can be readily solved to the great profit of all.

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BIBLIOGRAPHY

AMES, OAKES
1939 *Economic Annuals and Human Cultures.* Botanical Museum of Harvard University, Cambridge.

ANDERSON, E.
1943a *Races of Zea Mays: II.* Acta Americana, 1, no. 1, 58–68.

[22] Hugh Cutler, 1944, has given a classic example of how much meaning may be concealed in a seemingly obscure fact.

1943b *A Variety of Maize from the Rio Loa*. Annals of the Missouri Botanical Gardens, 30, pp. 469–474.

ANDERSON, E., AND F. D. BLANCHARD

1942 *Prehistoric Maize from Cañon del Muerto*. American Journal of Botany, 29, No. 10, pp. 832–835.

ANDERSON, E., AND HUGH C. CUTLER

1942 *Races of Zea Mays: I*. Annals of the Missouri Botanical Gardens, 29: 69–88.

BAILEY, L. H.

1929 *The Domesticated Cucurbits*. Gentes Herbarum, 2, facs. 2.

1930 *Three Discussions in Cucurbitaceae*. Gentes Herbarum, 2, fasc. 14, art. 8.

1932 Supplement, Gentes Herbarum, 2, art. 13.

1943 *Species of Cucurbita*. Gentes Herbarum, 6, fasc. 5.

BUKASOV, S. M.

1931 *The Cultivated Plants of Mexico, Guatemala, and Colombia*. Supplement to Bulletin of Applied Botany.

CARTER, G. F.

1945 *Plant Geography and Culture History in the American Southwest*. Viking Fund Publications in Anthropology, No. 5.

An early American Description Probably Referring to Phaseolus Lunatus, to appear in *Chronica Botanica*.

Some Archeologic Cucurbit Seed from Peru. To appear in an early issue of *Acta Americana*.

CARTER, G. F., AND E. ANDERSON

1945 *A Preliminary Survey of Maize in the Southwestern United States*. Annals of the Missouri Botanical Gardens, 32: 297–322.

CASTETTER, E. F., AND A. T. ERWIN

1929 *A Systematic Study of Squashes and Pumpkins*. Iowa Agricultural Experimental Station, Bulletin No. 244.

CUTLER, H. C.

1944 *Medicine Men and the Preservation of a Relict Gene in Maize*. Journal of Heredity, 35, No. 10, 291–294.

ERWIN, A. T.

1931 *Nativity of the Cucurbits*. The Botanical Gazette, 91,, No. 1.

1936a *Notes on Cucurbita moschata, Duch*. Iowa State College Journal of Science, 10, No. 3.

1936b *Nativity of Cucurbita maxima*. Iowa State College Journal of Science, 10, No. 4.

1938 *An Interesting Texas Cucurbit*. Iowa State College Journal of Science, 12, No. 2.

ERWIN, A. T., AND E. S. HABER

1929 *Species and Varietal Crosses in Cucurbits*. Iowa Agricultural Experimental Station, Bulletin No. 263.

GILMORE, M. R.

1911 *Uses of Plants by the Indians of the Missouri River Region*. Thirty-third Annual Report of the Bureau of American Ethnology.

IVANOV, N. R.

1937 *Geographical Regularities in the Distribution of Cultivated Phaseolinae*. Lenin Academy of Agricultural Science, Ser. 1, No. 2. (Manuscript translation in the Geography Department of the University of California.)

LONGELLY, A. E.

1938 *Chromosomes of Maize from North American Indians*. Journal of Agricultural Research, 56, No. 3, pp. 177–196.

MACKIE, W. W.
1943 *Origin, Dispersal, and Variability of the Lima Bean, Phaseolus Lunatus.* Hilgardia, 15, No. 1, pp. 1–24.

MANGELSDORF, P. C., AND J. W. CAMERON
1942 *Western Guatemala: A Secondary Center of Origin of Cultivated Maize Varieties.* Botanical Museum of Harvard University, Cambridge.

MANGELSDORF, P. C., AND R. G. REEVES
1939 *The Origin of Indian Corn and Its Relatives.* Texas Agricultural Experiment Station, Bull. 574.
1945 *The Origin of Maize: Present Status of the Problem.* American Anthropologist, n.s., 47, pp. 235–243.

MCKAY, J. W.
1932 *Cytological and Genetical Studies in the Cucurbitaceae.* Ph.D. thesis, University of California Library.

REEVES, R. G.
1944 *Chromozome Knobs in Relation to the Origin of Maize.* Genetics, 29, No. 2, pp. 141–148.

RUSSELL, P.
1924 *Identification of the Commonly Cultivated Species of Cucurbits by Means of Seed Characters.* Journal of the Washington Academy of Sciences, 14, No. 12.

SAUER, C. O.
1932 *The Road to Cibola.* Ibero Americana, No. 3, University of California Press.
1936 *American Agricultural Origins. In* Essays in Anthropology in Honor of Alfred Louis Kroeber, University of California Press.

SMALL, J. K.
1922 *Wild Pumpkin.* Journal of the New York Botanical Gardens, 23: 19–23.
1930 *The Okeechobee Gourd.* Journal of the New York Botanical Gardens, 31: 10–14.

TAPLEY, W. T., W. D. ENZIE, AND G. P. VAN ESELTINE
1937 *Vegetables of New York, Vol. 1, Part IV: The Cucurbits.* Reports of the New York State Agricultural Experimental Station for the Year Ending June 30, 1935.

VAVILOV, N. I.
1926 *Studies on the Origin of Cultivated Plants.* Trudy pro Prikladnoi Botanike, 16, No. 2.

VESTAL, P. A.
1931 *Cucurbita moschata found in Pre-Columbian Mounds in Guatemala.* Botanical Museum Leaflets, Harvard University, 6, No. 4.

ZHITENEVA, N. E.
1929 *The World's Assortment of Pumpkins.* Bulletin of Applied Botany and Plant Breeding, 23.

FURTHER NOTES ON TOBACCO IN NEW GUINEA

By E. D. MERRILL

IN 1930 I published a short paper on tobacco in New Guinea[1] in which I took exception to the more or less current belief among ethnologists, and apparently among some botanists, that tobacco was known to the aborigines of New Guinea long before the arrival of Europeans in the region, and also to the belief expressed by some ethnologists that the smoking of tobacco originated independently in New Guinea, perhaps on the basis of a native species of *Nicotiana*. I noted that apparently this myth or series of myths originated with Dr. O. Finsch, in 1886. However up to 1930 only a single species of *Nicotiana* (*N. tabacum*) was known from New Guinea, and the very extensive field work that has been prosecuted there since that date has failed to extend the list. All botanists and collectors who have considered the matter, that is, those that have actually been engaged in the exploration of New Guinea, insist that only a single species of *Nicotiana* occurs there, that this is always a cultivated plant, and that it is the common tobacco, *Nicotiana tabacum*. This is, of course, a species of American origin, but it may now be emphasized that it is definitely of hybrid origin. It is a cultigen, belonging in the same category as maize, in that for its existence it is largely dependent on man, having lost the ability to maintain itself in nature.

We now have access to the results of Dr. Lam's trip up the Mamberamo River to the central mountain range of Netherlands New Guinea, based on a full year of field work, and I have handled all of the extensive botanical collections made by Mr. L. J. Brass on the three Richard Archbold Expeditions, first in southeastern New Guinea from the coast to the summit of the Owen Stanley Range, second up the Fly River, and third from Hollandia to the central mountain range of Netherlands New Guinea. The total of the Brass collections approximates 10,500 numbers. Dr. Lam, in his year's experience in the field, saw only cultivated *Nicotiana tabacum*, and Mr. Brass encountered only forms of that species, some at low altitudes, and one from a relatively high altitude.[2] I have also had access to a partial set of the large Carr collection made in southeastern New Guinea, the Kanehira collections made in Netherlands New Guinea, and an extensive series of specimens collected by Mrs. Clemens in Northeastern New Guinea (German New Guinea), all made since 1930. All observers are in agreement that tobacco in New Guinea occurs only in cultivation, that its leaves form an important item in inter-tribal commerce,

[1] Merrill, 1930. (See bibliography at end of article.)

[2] The highest-altitude tobacco collected by Mr. Brass was his number 11394 from the Bele River, 18 km. northeast of Lake Habbema, at an altitude of 2,200 m., his note bearing the statement "cultivated by the Pesegem tribe." This is a form with relatively small, narrow, lanceolate, petioled leaves. It is a form of *Nicotiana tabacum* Linn.

and that cultivation occurs at both low and high altitudes where proper climatic and soil conditions permit. It is even found in cultivation among the primitive stone-age aborigines (the Pesegem and the Timorini) who inhabit the high and very remote interior valleys.

Dr. Lam reports that the Timorinese name for tobacco is *tabo* and that the term for the pipe is *tabaok*, and he correctly surmises that these were derived from the Malay *tabako*. The word is of American (Caribbean) origin and originally was applied to the cigar,[3] rather than to the plant. Like such names as tea and coffee the name *tobacco*, variously modified, was disseminated with the plant or its product to all parts of the world, and like tea and coffee this dissemination was modern and not ancient; that is, modern in the sense that the spread of the plants, their names, and their products followed the time of the discovery of America and the expansion of the European colonizing nations. Dr. Lam inferred, correctly in my opinion, that the occurrence of these names, *tabo* and *tabaok*, among the Timorini is an indication that the plant, with the name, reached the isolated interior primitive peoples of New Guinea through direct or indirect contact with the Malays. The Pesegem use *mbali* for tobacco and *kanoem* for the pipe. Dr. Lam cites Van Nouhuys' report that tobacco was not known by the tribes along the lower course of the Lorentz River, but only by the Pesegem, and notes the fact that *Nicotiana tabacum* is widespread in New Guinea.

Recently in checking certain botanical references in connection with our New Guinea plant studies, I noted a significant statement regarding tobacco in New Guinea, based on observations made between 1871 and 1877, and actually published in 1886, the same year that Finsch's unfortunate conclusions appeared. This statement is of so much interest that it is quoted in full below. From an examination of it one will readily perceive that the smoking of tobacco is clearly not ancient in New Guinea, and that we may safely infer that the practice was introduced by the white man, in relatively modern times, together with the plant itself, that in New Guinea the use of tobacco proceeded from the west to the east, and that it reached eastern New Guinea in the nineteenth century.

It is admitted that some botanists have reached conclusions corresponding to those of certain ethnologists regarding the occurrence of tobacco in some parts of the Old World in pre-Columbian times. Thus Dr. A. Chevalier[4] in 1927, perhaps misled by certain anthropological dogmas, concluded that the peoples of the southwestern Pacific region and those of southeastern Asia knew tobacco and the sweet potato long before the discovery of America by Colum-

[3] *Editor's note:* The general literature usually states that the original word *tabaco* referred to *pipe*. This is incorrect; pipes were unknown in the Antilles. (See J. A. Mason, *Use of Tobacco in Mexico and South America*, Field Museum of Natural History Leaflet 16, 1924.)

[4] Chevalier, 1927.

bus. Dr. H. Lam[5] discusses Chevalier's conclusions, taking a conservative attitude:

> These and other cultivated plants, according to Chevalier, through the agency of man, perhaps also by birds, and even on driftwood, might have extended via the numerous islands of the Pacific Ocean far to the west. I suppose that here [New Guinea] in particular the influence of man must be considered. Tobacco was first introduced into Java by the Dutch in 1601, but one might assume that it was known much earlier in the Moluccas and seemingly also in New Guinea. Van Nouhuys informs me that, from old travel records, it is apparent to him that tobacco occurred in Ternate in 1599, and that slaves (Papuans?) especially used it.

We know definitely that Rumphius was familiar with tobacco in Amboina between 1670 and 1690. Personally I do not think that we have to go back of the Portuguese colonization of Amboina in 1521 to account for the presence of tobacco in the Moluccas, and presumably also in New Guinea, before it was introduced into Java. After all, Van Nouhuys' date, 1599 for Ternate, is only two years before tobacco was introduced into Java and is nearly ninety years after the Portuguese actually reached Amboina, and seventy-eight years after they established a permanent settlement there.

It is a rather widely accepted belief that the sweet potato occurred in Polynesia and as far to the southwest as New Zealand long before the Europeans crossed the Pacific Ocean, the date of Magellan's arrival in the Philippines being 1521. If this important food plant did reach the above regions before the Magellan date one might expect that it may also have reached New Guinea. I have found no references that lead me to believe that the sweet potato reached any part of Papuasia, Malaysia or tropical Asia before the arrival of the Europeans. As to its early introduction into Polynesia and New Zealand see Messrs. Dixon[6] and Buck[7] *pro* and Friederici[8] *contra*.

Perhaps individuals may be impressed by the relatively large number of different local names applied by aboriginal peoples to manifestly introduced species. Yet these names should be used with caution when such data are utilized to support a theory of early introduction. It is clear that in many places native peoples have coined their own names on a distinctly large scale for certain introduced species which perhaps came to them without names, and yet soon became widely distributed in cultivation.

The situation in New Guinea with its numerous languages and dialects is

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[5] Lam, 1945, pp. 148–149. This is an English translation, by Dr. L. M. Perry, of the original Fragmenta Papuana, which was published in the Dutch language in the Natuurkundig Tijdschrift voor Nederlandsch-Indië, vol. **87** (1927) to vol. **89** (1929). It contains many data of interest to anthropologists and ethnologists regarding the primitive stone-age peoples who inhabit the high remote valleys of the interior of New Guinea, and their culture, particularly the Timorini and the Pesegem. [6] Dixon, 1932. [7] Buck, 1938, pp. 313–316.

[8] Friederici, 1929 (see long review by H. Damm, Ethnol. Anz. **3**: Referate 64–68, 1933), 1931, 1936.

peculiar. Miss Henriette Neuhaus of New York compiled for me the recorded native names for certain widely cultivated species in eastern New Guinea, including the yam, tobacco, and the sweet potato, from the annual reports of the Territory of Papua = British New Guinea, 1886–1927. Thus for tobacco, in excess of one hundred different names are listed as used in the Territory of New Guinea alone, with an even larger number of different names for the sweet potato. Clearly some of these names were based on those introduced by the Europeans, but in both cases the vast majority of them, to me at least, suggest no genetic relationship with names transmitted through Europeans.

For tobacco, by far the most widely used one, listed about sixty times, is *kuku*, this sometimes indicated as a trade name. But names apparently derived from tobacco include *bakuki*, *kamake*, *tabake*, *tabaken*, *tabaoak*, *tafaki*, *tampili*, *tapaki*, *tapai-e*, *taubaki*, *tobaki*, *tobako*, and *topako*, probably also *sakaba*, *sakapa*, *sakopa*, *sakupa*, *sekupe*, *sikube*, *sokuba*, *suguba*, *sukaba*, and doubtless others in the compiled list.

The variations in the names of the sweet potato and the yam, both, like tobacco, introduced plants in New Guinea, are just as striking, as is the case with the names of tobacco. The yam, being of Old World origin, was unquestionably of prehistoric introduction into New Guinea. Tobacco, certainly, and I believe the sweet potato, also represent introductions from tropical America in post-Magellan times. The New Guinea sweet-potato names are even more variable than are those of the tobacco, ranging from *atari* to *yembilyamagini*, with those most frequently recorded being *nai*, *kupe* and variants, *kaire*, *kailikuta*, *kanua*, *mosera*, and *nori*. It is of interest to note that the Quechua name, *kumara*, is recorded but twice, but some of the listed forms may have been derived from it.

Variants in the name *kumara* as actually used in the Pacific islands include the following: *gumalla* (Tahiti), *gumarra* (Tahiti), *kuma'a* (Marquesas), *kumala* (Fiji), *kumara* (Marquesas, Easter, Niue, Fiji, Rarotonga, New Zealand, New Guinea), *timala* (Niue), *uala* (Hawaii), *uma'a* (Marquesas), *umala* (Samoa), and *umara* (Tahiti, Austral Islands). Within Polynesia the name for the sweet potato seems consistently to have been derived from the Quechua *kumara*. The Spanish introduction of the species into the Orient from Mexico carried the Nahuatl name *camote*, this being the almost universal name for the species throughout the Philippines, and in Guam with such minor variants as *kamute* (Guam), *kamott* (Yap), and *kamolo* (Marshall Islands).

I am not unduly impressed by Friederici's statement, quoted below, insofar as possible Spanish introductions into the Solomon Islands, Santa Cruz, and the New Hebrides are concerned, for the simple reason that the early Spanish attempts to colonize those islands were abortive.

Friederici states:[9]

Hierzu gehört in erster Linie die Süsskartoffel. Ich habe nachzuweisen versucht

[9] Friederici, 1931, p. 140.

[footnote reference to Anthropos **24** (1929)], dass die Ipomoea Batatas Poir. nicht, wie bisher angenommen wurde, von den Südseeinseln aus nach Amerika kam, sondern dass vielmehr die in der Alten Welt bisher bekannte Batate von den Spaniern unter Mendaña [1595] und Quirós [1605] aus Amerika, wo sie altes alteinheimisches Kulturgewächs war, nach den Marquisas, den Solomonen, Santa Cruz- und Banks-Inseln und nach den Neuen Hebriden gebracht worden ist.

Clearly the wide use of the Quechua name *kumara* of Peruvian origin in Polynesia has impressed various individuals in favor of the idea that the plant with its name was a pre-Magellan introduction into Polynesia, for this name with minor variants occurs all the way from the Marquesas and Hawaii to New Zealand and even New Guinea, and the first European explorers of New Zealand found the plant in cultivation there under that name.

It is true that the Spaniards did discover the Solomon and Santa Cruz Islands at an early date, but they were not exploited. In fact for a period of two hundred years after the Solomon Islands were discovered the very existence of the group was doubted. Gallego sailed from Callao in 1566 and returned to South America in 1568, after exploring the Solomon group, but for some reason his journal was suppressed. It is now fortunately available in Guppy's[10] translation. To me the interesting point in this old narrative is that it is absolutely negative as to the occurrence of the sweet potato in the Solomons in the sixteenth century. Gallego does mention various plant foods that were brought to them by the natives, including *mames* or *names* and *panoes* or *panales*. The first was probably his name for the yam (*Dioscorea alata*), but Mr. L.J. Brass suggests a species of *Canarium* now known as *nali*, the seeds of which are very widely used for food in the Solomon Islands. From the context in Gallego's account one suspects that a root crop was involved. By *panoes* or *panales* Gallego manifestly was using not native names but some Spanish dialectal name for a European root crop, applied by him in the Solomons to the corms of the taro (*Colocasia esculenta*). There is no mention of the sweet potato. It may be assumed that, sailing as he did from Callao, he would have been familiar with the plant and with its Quechua name *kumara*.

Mendaña and Quirós did make a serious attempt to colonize the Solomons, sailing from Peru in 1595, in the course of this voyage discovering the Marquesas Islands. The expedition failed to reach the Solomon Islands, but did discover the Santa Cruz group. The attempt to establish a colony there failed, and after only two months the colonists left for Manila, with perhaps half of the original party of four hundred, which included both men and women, reaching that haven. Quirós' attempt in 1605 was just as abortive, for he never reached the Solomons, nor the Santa Cruz Islands, although he was in the vicinity of both groups. As both the Mendaña-Quirós and the later Quirós

[10] Guppy, 1887, pp. 102–245.

attempts were for colonization purposes it may logically be assumed that they were supplied with seeds and perhaps living plants of certain food-producing species, yet it is improbable, due to the short period of colonization involved, that they could have established such a species as the sweet potato in the island groups of the southwestern Pacific region that they visited.

If the sweet potato did reach the Pacific Islands in pre-Magellan times it could have been transmitted from America only by man, as it is not propagated by seeds, and in the tropics its tubers decay, under normal conditions, within a relatively short time. It may be that when it was introduced into Polynesia it may have been confused with the yam (*Dioscorea*), which the early Polynesians certainly did have, whether the introduction of the former be early or late. The sweet potato, once introduced, usually tends to dominate the yam in cultivation because of its superior qualities; and in some regions we know that aboriginal names of the yam, such as *ûbi*, were applied here and there to the sweet potato after the latter was introduced within post-Magellan times. Dr. Buck states that the sweet potato was in Hawaii by 1250 A.D., and in New Zealand by 1350 A.D.

I am afraid that in some cases individual authors may be accused of wishful thinking in that, at least in some cases, they have set up preconceived theories and have presented the data that supported a theory, ignoring those that are opposed to it. To be classed in this category are certain papers of O. F. Cook,[11] whose conclusions have been shown to be utterly erroneous.

The flotsam and jetsam idea as expressed by some to explain presumed early pantropic distribution of certain economic cultivated plant species through the medium of driftwood is so patently erroneous as to be somewhat humorous. The number of strand species that are of universal pantropic distribution is surprisingly small, and is confined to representatives of various families where the seeds float for indefinite periods in salt water and yet retain their viability. Not one of the numerous cultivated species falls in this category. Their seeds cannot withstand immersion in either salt or fresh water for more than a few days and still retain their viability, and few of them will even float. One has only to consider the seeds of such common species as the various cultivated cereals, beans, peas, cruciferous plants and others to realize their limitations. When this flotsam idea is applied to such a species as tobacco, a plant that for its continued existence, like maize, is dependent on man, one reaches the height of absurdity in postulating the driftwood idea.

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[11] Cook, 1901, 1910; Cook, O. F., & R. C. Cook, 1918. For data opposed to Cook's ideas see Beccari, O. Origin and Dispersal of Cocos nucifera. Philip. Jour. Sci. **12**: 27–43, 1917; Chiovenda, E. La culla del cocco. Contributio alla ricerca della patria originaria della palma di cocco. Webbia **5**: 199–294, 1921; 359–449, 1923; and Merrill, E. D. Comments on Cook's Theory as to the American Origin and Prehistoric Distribution of Certain Economic Plants, Especially Hibiscus tiliaceus Linn. Philip. Jour. Sci. **17**: 377–384. 1920.

Elsewhere I have discussed the problem of Polynesian weeds,[12] showing rather conclusively, on the basis of the botanical record, that there are two categories. The first includes those "Polynesian" weeds, that is, the relatively few species that were known to occur in Polynesia before 1782, and the second the very much larger number that have been introduced since that date. Most of those in the first category are definitely of Old World origin and represent early accidental introductions into Polynesia by the Polynesians themselves. In passing, Dr. Lam discusses the ruderal species (weeds) noted by him in the interior of New Guinea and comments on the relative paucity of such species in the paper referred to above. The second and much larger category represents the numerous species that have been introduced into Polynesia within the past 150 years. While some of these are of Old World origin, the vast majority of them came from America. My interpretation of the "Polynesian" weeds, i. e., the early introduced ones, was based on an examination of the Solander lists which in turn were based on the plants collected on Cook's voyages, 1769–82, these being the first botanical collections made in Polynesia.

This first category indicates little or no possible connection between Polynesia and America in early times. If there be a very few weeds of American origin in the early Solander lists, this is understandable because a great many early European exploring expeditions traversed the Pacific following 1521, these for the most part coming via the Straits of Magellan and the west coast of South, Central, and North America. It would be most surprising if the seeds of a few American weeds were not transmitted to various Polynesian islands in this period, for we know that a very considerable number of aggressive American weeds were introduced into Guam and into the Philippines through the intermediary of the Acapulco-Manila galleons between the latter part of the sixteenth century and the year 1815.

The de Miklouho-Maclay[13] observations that recently attracted my attention were made at Garagassi (1871–72) and Bongu (1876–77), near Port Constantine, in eastern New Guinea. The passage regarding the occurrence of tobacco in New Guinea, and one that, in my opinion, refutes the rather widely accepted conclusions of Finsch and others, is as follows:

"*Nicotiana tabacum* (*Kas*). The old natives of this Coast remember, that they were told by their fathers, that in their youth they (the fathers) were not acquainted with the use of tobacco and that the seeds and the knowledge of smoking have been introduced and have spread from village to village from the west. There are some villages in the mountains of the Maclay-Coast where the custom of smoking has not been introduced (1). The dried tobacco leaves are, before smoking, dried on a fire, after which they are torn, crushed and rolled in a leaf (2), also previously dried on fire, in the shape of a big cigarette and smoked. In some hill villages, the natives have large bamboo pipes (3), which

[12] Merrill, 1941. [13] de Miklouho-Maclay, 1886.

are filled with tobacco smoke from a cigarette and smoked by many people in turn, everyone trying to inhale and to swallow as much of the cold smoke as he can. The use of the pipe has not been adopted by the coast natives, who prefer to smoke cigarettes."

The footnotes by Baron Ferdinand von Mueller, the eminent Australian botanist, are:

"(1) The use of tobacco in the Louisiade Archipelago has been introduced quite lately. I visited in 1880 some hill villages on the Island Basilaki (or Moresby Island) where the natives were completely unacquainted with tobacco and smoking." (The Louisiade Archipelago is off the eastern end of New Guinea.)

"(2) The natives of the Maclay-Coast use the leaves of several plants as covering for their cigarettes. I am sorry however to say, that I have neglected to ascertain which are these special plants."

"(3) Just like on the South-Coast of New Guinea, where the use of tobacco, according to the authority of Rev. Lawes, has been introduced from the West and only lately has spread gradually to the S. East extremity of New Guinea and now the *kuku* (the native name for tobacco on the S. E. Coast of New Guinea) is in the greatest demand."

I can only infer from these statements, made by thoroughly competent observers somewhat earlier than Finsch's conclusions were published, that the growing and use of tobacco in New Guinea proceeded from the west to the east, which is exactly what would be expected if, as I believe, the Portuguese first introduced tobacco into the Moluccas, following the period of their establishment of a permanent settlement at Amboina in 1521. Amboina is not far from the west end of New Guinea. The idea that the use of tobacco in New Guinea may have been based on a native species is definitely erroneous, for no native species of the genus occurs in the island. Were Finsch correct in his belief regarding a much earlier introduction of tobacco into New Guinea via the Pacific islands in pre-Magellan times, then logically we would expect that its use there would have proceeded from east to west, and again, would it not be strange indeed if its use persisted in New Guinea, and yet became absolutely lost to the Polynesians? No early explorer of Polynesia mentions either tobacco or its use in Polynesia. Had tobacco been known in New Guinea "from time immemorial" one could only conclude that its use there would have been universal long before 1871–72, yet according to Van Nouhuys, as to some parts of New Guinea, it was unknown well into the present century. Thus there seems to be no reason for perpetuating the expressed belief on the part of some authors that tobacco occurred in New Guinea long before the Europeans arrived in the Pacific region, or that the art of smoking originated independently there. One concludes that it was the hardy European sailor who disseminated the tobacco-smoking habit, and that somewhat later, enterprising tradesmen actu-

ally introduced the plant into cultivation here and there in the orient with an eye to potential profits; for the smoking habit, once established, made tobacco a very potent article of exchange in bartering with primitive peoples. This conclusion may be in the nature of an anthropological or ethnological heresy, but it is the only one that I as a botanist can draw.

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BIBLIOGRAPHY

BUCK, PETER H.
1938 *Vikings of the Sunrise*. New York.

CHEVALIER, A.
1927 *Les origines du tabac et les débuts de sa culture dans le monde*. Revue Internationale Tabacs, 4, pp. 13–16.

COOK, O. F.
1901 *The Origin and Distribution of the Coconut*. Contributions from the U. S. National Herbarium, 7, pp. 257–293.
1910 *History of the Coconut Palm in America. Idem*, 14, pp. 271–342.

COOK, O. F., AND R. C. COOK
1918 *The Mako or Mahagua as a Trans-Pacific Plant*. Journal of the Washington Academy of Science, 8, pp. 153–170.

DIXON, R. B.
1932 *The Problem of the Sweet Potato in Polynesia*. American Anthropologist, 34, No. 1, pp. 40–66.

FRIEDERICI, G.
1929 *Zu den vorkolumbischen Verbindungen der Südsee-Völker mit Amerika*. Anthropos, 24, pp. 441–487.
1931 *Veränderungen in der Südseekultur seit der Zeit Mendanas*. Petermans Mitteilungen, 77, pp. 138–143.
1936 *Die Süsskartoffel in der Südsee*. Mitteilungsblatt der Gesellschaft für Völkerkunde, 7, pp. 2–7. Leipzig.

GUPPY, H. B.
1887 *The Solomon Islands and Their Natives*. London.

LAM, H.
1945 *Fragmenta Papuana* [*Observations of a Naturalist in Netherlands New Guinea*]. Sargentia, 5, pp. 1–196, 32 figs., 2 maps.

MERRILL, E. D.
1930 *Tobacco in New Guinea*. American Anthropologist, 32, No. 1, pp. 101–105.
1941 *Man's Influence on the Vegetation of Polynesia with Special Reference to Introduced Species*. Proceedings of the Sixth Pacific Scientific Congress, 4, pp. 629–639.

MIKLOUHO-MACLAY, A. DE
1886 *List of Plants in Use by the Natives of the Maclay-Coast, New Guinea (with Botanical Remarks by Baron Ferd. von Mueller)*. Proceedings of the Linn. Society, N.S. Wales, 10, pp. 346–354.

DIFFERENTIATING MYTH, LEGEND, AND HISTORY IN ANCIENT CHINESE CULTURE

By MAURICE T. PRICE

AS long as a great body of myths, legends, and historical facts about a people remain undifferentiated in a total mass of credulously accepted tradition, oral and written, that people can be understood by neither itself nor by others.[1] In the case of some of the chief peoples of the Near East and of ancient Europe, the labor of distinguishing between legend and historical fact, in particular, has been a long and arduous one. In the case of many of the peoples of the world, however, as we have been most forcefully reminded of late, it is not merely legends and a people's legendology (if we may coin the word) that must be disentangled from historical events and conditions, in the minds of its own populace, but also myths and a people's mythology. This is notoriously true in the case of Japan's Sun-goddess mythology, which has been calculatingly supported for nationalist and politico-military purposes. Less widely recognized, however, is the fact that the understanding of China, both by its own populace and by others, the understanding of Chinese culture and of China's potentialities for socio-cultural development is seriously affected by various degrees of confusion in this entire sphere of myth, legend, and historical data.

Those familiar with translations of Chinese literature know how permeated it is with allusions to mythological and legendary figures, as well as authenticated historical ones. Acquaintance with the renowned *Classics* shows them to be saturated with references to the personages and conditions in the haze, shadows, and even mirages of tradition. Most important of all for the hard-bitten realist of today trying to help shape a world in which peace may supplant war, the leaders of the New China arising out of the twentieth century have gone back, time and time and time again, in their defense of policy and program, to the precedent and sanction of the Ancients. Chiang Kai-shek's speeches to his fellow-countrymen, calling for unity and perseverance and a world view, sometimes build elaborately on a foundation of Chinese history and tradition. The speeches and writings of Chinese interpreters of their people to the citizens of the United States and the rest of the Occident, like Lin Yu-tang, trying to impress upon us the humanistic and liberal convictions in their culture, lean heavily upon the traditions embodying the *Confucian Classics* themselves. And American interpreters of the Chinese to the citizens

[1] It is admitted that the mere fact that sections of a people are credulous and naive enough to believe myths, and that their legends have certain significant meanings for them, does aid others in understanding that people; but whoever makes such a judgment, obviously, has already differentiated between myth, legend, and historical events and conditions, and is able to make such a judgment on the basis of that differentiation.

of this country and the West generally, attempting to arouse our confidence in the policies and programs putting the Chinese people among the world's great democracies, may go back "at least three thousand years ago" to traditions of the "direct popular election" of local magistrates.[2]

Not for a moment is it contended that all these speakers and writers *confuse* myth, legend, and historic fact in their presentations. Rather is it contended that, since differentiation of them is found among some interpreters of China, and confusion of them is found among others, the intelligent public in a country like the United States has no option but to be informed sufficiently on the subject to be able to demand some sort of credentials from those who quote ancient Chinese history to support their analysis of Chinese society and thought, or to validate their interpretations of trends and prospects in modern times.

THE CONFUSION OF MYTH, LEGEND, AND HISTORY

The confusion which must be borne in mind is exposed in preliminary perspective in the hypothesis of Ts'ui Shu (1740–1816), the most distinguished modern representative of unorthodox historical critics prior to the 19th and 20th centuries. His proposition was that the lore presenting the most ancient Kings and Model Emperors of the third millennium B.C. and before not merely was myth and legend, but actually had been "built up in successive strata, so that the more remote from a given event, the more detailed becomes the information about that event. Thus Yao and Shun" [the first two of the Model Emperors, allegedly 24th and 23rd centuries B.C.] to continue quoting from Hummel's account of Ts'ui Shu's findings, "are unknown to the earliest classic, the *Odes* [10th–7th centuries B.C.?]; Shen Nung [the 'Divine Husbandman' and father of medicine, allegedly 28th century B.C.] appears first in the writings of Mencius [latter 4th to latter 3rd century B.C.]; the Huang Ti lore [Huang Ti was the great 'Yellow Emperor,' allegedly 27th century B.C.] first becomes prominent in the Ch'in period (255–206 B.C.); and P'an Ku [mythical creator of heavenly bodies and earth's living things], supposedly the most ancient figure of all, is mentioned first in the literature of Han [206 B.C. –220 A.D.]. Ts'ui Shu also pointed out that the writers of antiquity made a practice of substantiating their theories by illustrations drawn from folklore. After long transmission these illustrations, together with accretions and mistaken interpretations, were accepted as fact, thereby vitiating many histories, commentaries, and philosophical writings that appeared after the time of the Warring Kingdoms (403–255 B.C.)."[3]

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[2] Lacy, 1943, pp. 20–21. As the blurb on the cover puts it rather aptly, the author's contention seems to be that "China has been a democracy longer than any nation on earth, and is today the truest of the democracies. Though the form of democratic government as we know it is not there, the spirit prevails." (See bibliography at end of article.)

The chronological spotting of the rise of belief in a very prominent traditional personage, which was so emphasized by Ts'ui Shu in connection with this "successive strata" hypothesis, has now come to be a more or less familiar procedure. Almost any student of ancient Chinese literature and history may now work out a set of allusions and specific references which for him illustrate the principle that, for a considerable period, later and later generations came to believe in earlier and earlier traditional personages. It may be well, therefore, to supplement the above instances taken from Ts'ui Shu with others which even the amateur, coming from another field into that of Chinese history, might be expected to know. Here, therefore, is such a set of three major references with a little pertinent background. First of all, in one of the oldest sections of the *Analects*, Confucius (presumably c. 551–479 B.C.) is represented as saying, "I have 'transmitted what was taught me without making up anything of my own,' " and as refusing to admit that anyone excelled him as a veritable Nestor devoted to the Ancients.[4] The acceptance of these statements at their face value has led many a person to accept Confucius as authority for belief in the illustrious Model Emperors Yü (traditionally asserted date of accession, 2205 B.C.), the earlier Shun (asserted accession, 2255 B.C.), and, still further back, Yao (asserted accession, 2356 B.C.)—all of which are assumed in the Confucian *Analects*.[5] Second, if the Chinese or foreign student accepts at their face value the *Historical Records* of Ssu-ma Ch'ien (145–87 B.C., in the Han dynasty—thus several hundred years later than Confucius), which comprise "the first general history of a consolidated China" by "the outstanding individual historian of Chinese antiquity",[6] he must believe that those ancient Emperors in turn were developing a cultural heritage coming down from the still earlier Haung Ti, or Yellow Emperor (traditional accession, 2697 B.C.). Third, if one also accepts the noted third century B.C. Confucian, Hsün Tzu, one must believe that all the foregoing personages continued the heritage passed on by the still earlier Shen Nung (traditional accession, 2737 B.C.) and his predecessor Fu Hsi (traditional accession, 2852 B.C.).[7]

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[3] Hummel, 1931, pp. xxvii, xxviii. In general, see Gates, 1932, and 1936. See also Granet, 1930, p. 9. Data and remarks in brackets are those of the present writer.

[4] Waley, 1938, VII, 1, 2.

[5] All three Emperors also appear as actual historic figures in the *Classic of History* which was supposedly edited by Confucius and "parts of which were believed to date back to the twenty-third century B.C." See Hummel, 1932, p. 383 Ab.

[6] *Ibid.*, p. 383 Bd.

[7] This would follow, also, if one accepts as historical *all* parts of the *Classic of Changes*, another of the *Confucian Classics* supposedly edited by Confucius, for parts of it also refer to these last two personages as Emperors.

CONTINUED ACCEPTANCE OF BELIEFS IN TRADITIONAL PERSONAGES AND CULTURAL ACHIEVEMENTS

The vividness of the widespread belief among even Chinese scholars in the great politico-military and civilizing role of Huang Ti, the Yellow Emperor, is impressed on us by the noted historical critic Ku Chieh-kang who asks rhetorically, "Did not the official bulletins that were posted on every street and lane in the days of the Revolution [of 1911] state clearly that we were living 'In the year of Huang-ti 4609'?"[8] Nor is continuing reminder lacking that traditional Chinese historians had put a similarly effective seal of approval upon the belief in the still more remote personages Shen Nung and Fu Hsi. Thus, such a typical modern Chinese author as Wen Kwei Liao not only declares that "the actual history they [the ancient Chinese] accepted with authority and authenticity began with the record of Fu Hsi, the first of the Five Emperors," but himself presents, with virtually no strictures of an historical-critical nature, the achievements of these emperors as a background for understanding the social order and social thought in ancient China.[9]

A procedure similar to that of Liao could be cited from an assortment of Chinese authors ranging from the most pedestrian to the brilliant and eminent. They are *not* likely to draw sharp lines between history and legend (not to speak of legend and myth), fact and fiction, at the places where lines would be drawn by a college-trained Westerner who may have his own kinds of myths and legends to be sure, but who comes to the Chinese ones freshly. Bear in mind that Fu Hsi, for instance, is reputed himself to have had dragon's scales and dragon counsellors, and that Shen Nung is reported to have had the head of an ox. The fact seems to be that the typical Chinese writers of even the recent generation were apparently unperturbed by the fabulous myths and the diverse legends attached to the most ancient personages, being aware that even such later historical figures as Confucius were honored with sainthood. In general therefore they have seemed to exercise what the reader might call some freedom of choice in what they accept and relay to others; and at times they have appeared to suit that choice somewhat to their purposes and their audiences rather than to a trained historical and anthropological sense. Moreover, where they do *not* accept *episodes* or *traits* which are conflicting and—from the Western viewpoint—fantastic, they may nevertheless believe and pass on to others accounts of the rich accumulations of inventions and social institutions reported in time-worn documents as having occurred in the third millennium B.C., and so Mr. Liao has really not been exceptional in presenting the cultural achievements of the Five Emperors.

This general position, attitude, and procedure were taken over by many an

[8] Hummel, 1931, p. 81.

[9] Wen Kwei Liao (1931? and) 1933, p. 143 ff. See also p. 141 for his summary in a bona fide manner of the chief points in their culture in the 27th century B.C. His Five Emperors include Fu Hsi, Shen Nung, Huang Ti, Yao, and Shun, but not Yü.

Occidental student, in spite of the studious labor of the so-called Western Orientalists. In some cases, not having read the complete accounts themselves in which the fantastic elements appeared, such a student was not conversant with the mythological (and legendary) setting in which the record of the culture had appeared. However it may have happened, the fact is that accounts of cultural achievements of the third millennium B.C. appeared in numerous Western books, even in books by lecturers on the Far East in larger American universities. And thence in turn they finally came to be retailed by Occidental writers on the history of social thought in our own immediate academic milieu. The following summary, to illustrate, is presented in a matter-of-fact manner in a chapter on "The Social Thought of Ancient China" in a book first appearing as late as 1936 by Joyce O. Hertzler:[9a]

> The culture of ancient China was an unusually advanced one at a relatively early time in world history. More than twenty-five centuries before the dawn of the Christian era and nearly twenty centuries before the founding of the city of Rome, China was possessed of a civil and criminal code, statute laws, nine departmental ministers of state under the emperor, extensive home industries, a large import and export trade, a systematized canal and river service, a standing army and extensive agriculture, local government and tributary taxation, and schools of art, science, and music under the patronage and protection of hereditary dukes and other people of rank. To this astonishing list of cultural accomplishments must be added a well-developed writing, a remarkably advanced astronomy, a calendar, the magnetic needle, mathematics, the metric system of weights and measures, musical science with its instruments and notation, agricultural implements, cooking utensils, herbal medicine, silk production, weaving, drawing, painting, engraving, pottery, bookkeeping and accounting, engineering, embracing many principles of physics, and, as hinted above, the writing of essays, fiction, and poetry.[10]

Such is the greatness of China's ancient past accepted in varying degrees by Chinese traditionalist and uncritical Occidental interpreter. No wonder Hertzler calls it "astonishing."[11]

BASES OF CREDULITY

To explain at all thoroughly the acceptance of such a fictional reconstruction of the cultural heritage of China at that time would require, if space permitted, the consideration also of such factors as the dominant position of traditionalism, its continued inculcation and relative monopoly of attention, its prestige, its use as a psychological reinforcement for current behavior, and, in the case of Occidentals, analogous traditions.

One clarifying fact to keep in mind is that Ts'ui Shu, and his predecessors and followers, have been unorthodox. They have been scholar-heretics, heretical scholars. The great body of Chinese scholars has passed on legend and indeed myth as part of China's great undifferentiated tradition, without press-

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[9a] Hertzler, 1936. [10] Hertzler, 1936, pp. 201–202. [11] *Ibid.*

ing fresh questions of historicity or authorship. Chinese Classics, like the early writings of most or all ancient "literate" peoples, are composite documents actually made up of parts written at different times by different persons, edited, emended, and altered by later and later writers. And, partly in common with the ancient scribes of some other peoples, orthodox Chinese scholars had certain specific customs, characteristics, and points of view which were utterly at variance with those required by the standards of contemporary historical scholarship. Take for instance the early Chinese characteristic of assigning one's ideas or writings to "Sheltering Worthies" of the past[12]—a trait often obscured perhaps under the generalized charge of "forgeries" and "spurious literature."[13] Another customary trait of great influence has been the writing of (Chinese) history with an eye to its moral effect.[14] Associated with that trait, moreover, is the point of view which conceives of "the materials of history as all on the same plain, that is to say, horizontally" instead of vertically.[15] And, of course, there is the familiar tendency to solidify beliefs about ancient documents and their interpretations, into orthodoxies.

Finally, even a preliminary study of Chinese historiography makes it evident that the present generation has no monopoly upon him whom we might call the cultural patriot, upon the conservative to whom ancient "historic" precedent lends sanctity, or indeed upon the radical who seeks reinforcement from history. To all such, however, it is of great importance, as we have indicated above and in a paper on "The Isolation of China and Autochthony of Her Culture,"[16] that the institutions and customs advocated can be attributed to a remote past. Particularly is this true in a country like China where the prestige of ancientness and of great personages has been used for over two millenniums with a compulsive (and indeed propagandic) power which has won for it the term "reverence." And in recent decades, as illustrated from the field of continual moral criticisms of the Chinese Government as well as the field of specific reforms discussed in the cited paper, that prestige of ancientness and of great personages has remained powerful. Where precedent can be quoted from the Great Philosophical Period (6th to 3rd centuries B.C.), *that* is done; but where it can be pushed back to early Chou times (12th or 11th to 8th centuries B.C.), better to the Golden Age of Yao, Shun, and Yü (24th to 22d centuries B.C.), and better still to the previous three *Ti* or Supreme Ones, *that* is done.

Some of this use of precedent and sanction is more easily appreciated by contemporary Occidentals than other aspects. When ancient historical prece-

[12] Waley, 1934, 1942, 102 ff.

[13] E.g., see Ku Chieh-kang's autobiography, *passim*, in Hummel, *op. cit.*

[14] *Ibid.*, p. xvii of Hummel's Introduction; also Hummel, 1932, p. 383; and Gardener, 1938, pp. 13–14.

[15] Ku Chieh-kang in Hummel, 1931, p. 81; also Hummel, 1932, p. 383, on the elimination of the concept of change, and on the dominance of the concept of history as static ideas.

[16] Price, 1945.

dent is appealed to for the fixing of political boundaries, as in the case of Chinese claims to northern Korea and southern Manchuria, it becomes intelligible to almost any contemporary Occidental.[17] However, if the Westerner is to understand with any vividness the various other roles of the remote past in recent and current Chinese thought, he will probably have to maintain a comparative point of view that includes the Hebrew-Christian and perhaps Greek traditions. He may need to recall the appeal which the Jewish Rabbi, the Christian preacher, and the devotee of the Greek cultural heritage make to the personages, the way of life, and the ideals of conduct in their respective traditions, ancient and revered. And he might do well to recall the fright and at times the terror with which Biblical literalists looked upon the historical criticism of the Bible, and upon those who relegated some accounts of the most ancient carriers of Biblical sanction to the status of *Folklore of the Old Testament*. He may need to consider the fact that somewhat similar attitudes have produced resistance to applying critical historical inquiry to *all* revered tradition, whether Hebrew-Christian, Chinese, or Hindu. And finally, he may need to realize that, partly as a consequence of this, partly on account of the complexity of the critical problems involved, and partly for other reasons, all this tradition was long taught (after being established as orthodox, of course), with little distinction between legend and history. Indeed, in the records of the ancient Eastern cultures most familiar to us Occidentals, the lines between legend and bona fide history, not to speak of the lines between myth and legend, are still hazy to most of us.

No wonder then, that in referring to ancient Chinese history, many a popular writer or lecturer on the Far East is still liable to make a bow to the sceptre of historicity, and then to abstract or quote *ad libitum* either from primary sources without criticism, or from secondary sources without checking up on whether the authors of these pay any real obeisance themselves to that sceptre. And compiler-historians of social thought appear to be no exception in essential parts of their treatments of the Far East.[18]

THE CUMULATING CRITICISM COMES TO GRIPS WITH THE CLASSICAL SAGES AND THEIR TEACHING

It should now be fairly comprehensible, in a preliminary way, how it came about that, approximately two millenniums ago, orthodox Chinese scholars

[17] See Hsü, 1926, pp. 1, 2, emphasizing that "northern Korea and southern Manchuria formed a part of the Chinese world at the *very beginning* of Chinese history" (italics mine), and yet merely referring to the fact that "students of the Chinese classics claim that long before the reign of Yao" those areas "had formed a part of two of the nine provinces into which China was then divided," and that "at the time of the ruler Shun the regions in question were organized into two separate provinces . . . recorded to have been added to China by that ruler."

[18] E.g., Hertzler, 1936, in footnoting the above-quoted paragraph by referring to pp. 19–22 of H. H. Gowen and J. W. Hall, *An Outline History of China*, 1926; also in our parallel article, Howard Becker in taking such material from Creel, Hertzler, and others for his chapter on "Social

credited to prehistoric times and to prehistoric personages a great composite of chronologically later inventions, social organization, and conceptions. In a very general preliminary way, also, it should be understood why the resultant mixture of myth, legend, and history appeared plausible or convincing to later Chinese scholars.[19]

While attempting thus to envisage this general stream of orthodox traditionalism it is important, on the other hand, that we do not minimize the long-persisting nature of the lesser stream of *un*orthodox criticism, even though that stream was wont to disappear into the sands of time for generations or even centuries before it reappeared in any considerable volume. Ts'ui Shu was not alone. While deserving of signal prominence and fame, he was but one of "an unbroken line of critical [Chinese] scholars who had attacked spurious elements in Chinese literature." A roll of some of the most eminent of these unorthodox scholars is called by Ku Chieh-kang; another list is offered by his American sinologist translator, Arthur W. Hummel.[20]

Nor can we afford to lose sight of the more recent resurgence of these cumulative demands for redating, re-evaluating, and regrouping the component parts of ancient Chinese tradition. As a result in large part of this recent development, it is not alone the myths, legends, and pseudographs regarding Chinese origins and the Model Emperor Lore of the third and second millenniums B.C. which have been re-evaluated so drastically as to influence the traditional sanctions of modern movements. With the renewal of critical scholarship in China at the end of the nineteenth century and its flowering out into the so-called modern cultural renaissance under the leadership of K'ang Yu-wei, Liang Ch'i-ch'ao, Hu Shih, Ku Chieh-kang, and others, even the first millennium B.C. has come under the most rigorous examination. This Chinese or native scholarship and renaissance has been supplemented by the work and stimulus of such "foreign" scholars as Edouard Chavannes and other continental historians, as Bernard Karlgren and more recent linguists and philologists, as Marcel Granet and other social theorists.[21] Under the combined efforts of Oriental and Occidental, the formerly sacred accounts of the personages, events and culture traits of the great Classical period of that first millennium have come under the blade of historical, textual, and philological criticism. Under increasing knowledge of the processes through which ancient Chinese documents arose and were given successive forms, and with the application of distinctions between myth, legend, and authenticated history, teachings once

Thought of the Ancient Far East" (Ch. 2) in Vol. 1 of Barnes and Becker, 1938. Other historians and social scientists have done the same, of course.

[19] No pretension has been made by the writer to offer any inclusive or selected set of references on the general topic of authorship, historicity, manuscripts, orthodoxy, etc., in ancient China. It might be well, however, for the novice in China's historical and textual criticism to read "Authorship in Early China . . . ," pp. 101 ff. in Arthur Waley, 1934, 1942.

[20] Hummel, 1931, pp. xxii ff., 75d, 151.

[21] While this Occidental scholarship has been chiefly Continental-European, British, and Scandinavian, our American historians and social scientists have been duly informed of it in their

secure under the toga of orthodoxy have been transformed and the biographies of their supposed teachers emasculated.

It may not be altogether wise to cite single instances because they may give a wrong impression. This is bound to be true where the field is so extremely complicated, where there is at present a great diversity of opinion, and where there probably will be such diversity for some time to come. But, if merely for illustration, something must be said about Lao Tzu and about Confucius and his supposed teachings. It is traditionally held that the author of the famous Taoist classic, *Tao Te Ching*, was an older contemporary of Confucius by the name of Lao Tzu (c. 604-514 B.C.?), as portrayed by the Chinese historian Ssu-ma Ch'ien of the first century B.C. This assumption, it is now strongly maintained in spite of its support by Hu Shih, is no more tenable *in toto* than the traditional biographical account of Confucius by the same historian, if in fact it is anywhere as nearly tenable. The portrait of Lao Tzu, in brief, is asserted to be a composite from partially contradictory legends and reports about figures out of several different centuries. And as for the traditional reports on Confucius and his teachings, they are asserted to constitute, not one Confucius with one set of teachings, but a number of successive Confuciuses—as Ku Chieh-kang puts it—with successive sets of teachings ascribed to them.[22] The diversity in conceptions of Confucius did not end with the classi-

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own tongue. As early as 1904 an American audience at the St. Louis World's Exposition was told by the editor and founder of the *Revue de l'Extrême Orient* and the *T'oung-pao*, Henri Cordier, that "before the eighth century B.C., no solid basis to the history of China is to be found." (H. J. Rogers, ed., *Congress of Arts and Science*, Boston, 1906, II, p. 87.) In more recent years as American sinology was emerging, Prof. Kenneth S. Latourette and Dr. Arthur W. Hummel, chief of the Orientalia division of the Library of Congress, have taken pains to try to inform American scholars of the development of the critical historical movement in China. (Aside from the publications noted in footnotes 3 and 5, see Latourette, "Chinese Historical Studies During the Past Nine Years," *American Historical Review*, 1930, and similar article in 1921; also Hummel, "The New Culture Movement in China," *The Annals of the Amer. Acad. of Pol. and Soc. Sci.*, 152 (Nov., 1930), pp. 55–62, and especially "What Chinese Historians Are Doing to Their Own History," *American Historical Review*, 34 (July 1929), pp. 715–724.)

[22] To typify the more recent views about Confucius himself, it is perhaps fitting to call in the broad-sweeping historian of Chinese thought, Fung Yu-lan. After summarizing the biography of Confucius in Ssu-ma Ch'ien's famous *Historical Records* (*Shih Chi*), Fung Yu-lan asserts: "Such is the concept of Confucius that has held sway in China for the last two thousand years; yet a study of this biography, today, reveals many points which cannot possibly be in accordance with facts. It nevertheless remains noteworthy that Ssu-ma Ch'ien (145–c. 86 B.C.), the author of *Shih Chi*, still regarded Confucius as a human person at a time when many were already coming to look upon him as a divine being." (See Fung Yu-lan, *A History of Chinese Philosophy*, translated by Derk Bodde, 1937, p. 46.) On the earliest versions and interpretations of Confucius, one might well refer to Arthur Waley, 1938, pp. 13–14. Regarding Confucius' supposed connection with the so-called Confucian Classics, Arthur Hummel declares: "The tendency is to disassociate the name of Confucius from every one of the ancient classics, and to assume that his sole connection with them was to use them as text-books for the exposition of his social and political ideas." (Hummel, *Amer. Hist. Rev.*, 34: p. 722.) Of Confucius' authorship of the *Analects* in particular Dr. Hummel writes: " . . . Ts'ui Shu [eighteenth century] brought forward convincing proof to

cal Chou dynasty, moreover, or with its final Period of the Contending States. It is claimed by Liang Ch'i-ch'ao that, beginning with the State's official elevation of Confucius' teachings in the Han period (206 B.C. to 220 A.D.), "Confucius has undergone a series of transfigurations throughout these centuries."[22]

Whether therefore it be (i) the facts as to Confucius' life, character, and mode of living, (ii) his actual teachings, (iii) his supposed editing of ancient *Confucian Classics* and writing of parts of them, or finally (iv) the effects and influences of his life, character, work, and teachings upon posterity—whichever it be, one can no longer legitimately narrate Confucius' life and expound Confucianism without a broad acquaintance with the results of scholarship in this entire sphere and without appropriate qualification, reservation, and reference to differing authorities.

SOME CONCLUDING IMPLICATIONS

The modern reassessment of ancient Chinese culture and thought may be said to have developed chiefly in connection with two problems, that of the autochthony or indigeneity of the culture, and that of the extreme ancientness of the culture. Elsewhere, as suggested earlier, we have discussed the assumption that ancient China was virtually isolated from other countries of the world and that her ancient culture was therefore virtually autochthonous. There we have indicated the way in which this assumption permitted her ancient culture to acquire an added uniqueness of authority which still today acts as a powerful sanction. But we have seen, on the other hand, the need of revising that assumption and therefore of reassessing the foundations of her cultural heritage on that ground.

In the present paper we have also dealt with time-worn sanctions rooted in China's cultural heritage. And, too, we have considered a re-estimate of those sanctions, based in this case on a reassessment of the age and development of the culture. Indeed our inquiry has taken us back to some of the same orthodox historians who figured in the notion of the indigeneity of the culture. In

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show that not a little of the material in the last five of the twenty sections of the *Analects* belongs to a later time. Certain misinterpretations of the life and character of Confucius, which are now current in the West, can be traced to irrelevancies and misstatements of fact that appear in these sections. Moreover, in the fifteen authentic sections there are certain closing paragraphs which have suffered corruption in the process of transmission." (*Ibid.*, p. 721.) More recent critics, of course, both Chinese and foreign, such as Ku Chieh-kang and Arthur Waley, have carried these earlier studies much further.

[22] Continuing, he writes, "First, he became a Tung Chungshu and a Ho Shaokung; next, he appeared as a Ma Chich'eng and a Cheng K'angch'eng; then suddenly a Han Yu and a Ouyang Hsiu; then a Ch'eng Hsi and a Chu Hsi; again a Lu Chiuyuan and a Wang Yangming; and finally a Ku T'inglin and a Tai Tungyuan. All these schoolmen were obsessed by some aspects of Confucianism and unable to beat new paths of thought. Such is the tragic consequence of two thousand years of Confucianism as state doctrine!" (Quoted without source in Lin Mousheng, *Men and Ideas*, 1942, p. 229.)

this case, however, the problem has revolved about the credibility of the existence and/or extreme ancientness of certain personages, certain ideas, certain activities, certain conditions. For, the alleged fact and/or extreme ancientness of these personages, ideas, activities, and conditions, have given and still give prestige to contemporary ideals, policies, and programs patterned after them. But here too, as in the case of the alleged autochthony, accumulated and recent knowledge has been undermining the assumption and contention of the orthodox historians.

The fact is that, just as an acquaintance with the geographic distribution and migration of plants, animals, and humans, and of humans' artifacts, techniques, and lore, over the face of the earth was certain to act as a restraint upon scholars' readiness to attribute indigeneity to various component features of Chinese culture, so an increasing acquaintance with the use of canons of historical and textual criticism[24] was certain to act as a restraint upon the credulous acceptance of China's traditional version of its own past. While contrasted with each other, these two approaches are in a sense complementary, being different modes of inquiry through which ancient cultural achievements have been deflated. They are likely therefore to operate together upon orthodox traditionalism. For perspective it is necessary to add that China is not, of course, unique in this respect. The same two complementary approaches have tended to affect traditionalist contentions similarly in the case of other great ancient cultures on the literate level. This broad comparative outlook, however, should not be allowed to obscure the concrete and unique results of historical and textual criticism in each individual cultural area.

The implications of the archeological and historical studies bearing upon Chinese culture may seem embarrassing for the Oriental and Occidental social scientist and historian of social thought who have been passing on the orthodox accounts of Chinese sages and their teachings. To illustrate from the widely accepted accounts of ancient Chinese culture and thought mentioned earlier, the traditional biographical materials on Lao Tzu and Confucius presented by Hertzler and taken from Hertzler by Becker,[25] obviously need revision. Their traditional historical placement of these Sages' teachings in whole (*Tao Te Ching* in particular, for instance) and in part (Confucius' *Analects* especially), require restatement. The specific traditionally accepted teachings they quote must be re-evaluated in view of the different sub-documents or emendations in which they are found, and the probable authorship of these latter. And their estimates of these Sages as persons and teachers cannot but be affected accordingly. It is a little ironical that Becker remarks with unwitting force and applicability, "Confucius is a much misunderstood figure."[26] So also is Lao

[24] Or, more broadly, an acquaintance with the inclusive social and technical processes of oral and written transmission of objects, techniques, and ideas, not omitting the changing meanings of ideas during this transmission.

[25] Hertzler, 1936, ch. VII, *passim*. Howard Becker, in Barnes and Becker, ch. II, *passim*.

[26] *Op. cit.*, I, p. 58.

Tzu. Students of comparative culture cannot avoid the responsibilities of appreciating such misunderstanding in a much more thorough going manner than have these interpreters.

Just as striking, and far more significant in their import for China, moreover, are the implications of all this historical scholarship for the use of ancient Chinese records for present-day policy and program in China. The use of these eminent Sages and their teachings, Confucius' in particular, as clues, norms, and sanctions applicable for current principles and socio-economic-political programs in China can no longer be justified by quoting excerpts from the *Analects*, other supposed collections of Confucius' teachings, or Ssu-ma Ch'ien's biography of him, with indiscriminate and uncritical disregard of their historicity.

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BIBLIOGRAPHY

BARNES, H. E., AND H. BECKER
1938 *Social Thought from Lore to Science*. Boston and New York.

GARDENER, CHARLES S.
1938 *Chinese Traditional Historiography*.

GATES, M. JEAN
1932 *The Model Emperor Lore*. (Master's Thesis) Columbia University.
1936 *Model Emperors of the Golden Age in Chinese Lore*. Journal of the American Oriental Society, 56, pp. 51–76.

GRANET, MARCEL
1930 *Chinese Civilization*. New York.

HERTZLER, JOYCE O.
1936 *Social Thought of the Ancient Civilizations*. New York and London.

HSÜ, SHUHSI
1926 *China and Her Political Entity*. New York.

HUMMEL, ARTHUR W.
1931 *The Autobiography of a Chinese Historian*. Leyden.
1932 *History and Historiography*. Encyclopaedia of Social Sciences, VII. Section on China and Japan.

LACY, CREIGHTON
1943 *Is China a Democracy?* New York.

LIAO, WEN KWEI
1931?, 1933 *The Individual and the Community*. International Library of Psychology, Philosophy, and Scientific Method. London and New York.

PRICE, MAURICE T.
1945 *The Assumed Isolation of China and Autochthony of Her Culture*. American Sociological Review, 10, No. 1, pp. 38–43.

WALEY, ARTHUR
1934, 1942 *The Way and Its Power*. Boston and New York.
1938 *The Analects of Confucius*. London.

THE RICE GODDESS AND THE FOX IN JAPANESE RELIGION AND FOLK PRACTICE

By MORRIS E. OPLER *and*
ROBERT SEIDO HASHIMA

IN the latter part of March, 1943, I joined the War Relocation Authority and for eighteen months thereafter held the position of Community Analyst at the Manzanar Relocation Center. Since one-third of the evacuees who were living in the Center were immigrants who had begun life in Japan, and some of the young people, though American citizens, had lived in Japan and been educated there (these are the much-discussed *kibei*), this proved an excellent opportunity to gather information about Japanese customs and beliefs. The pressure of events was so great, however, that it was a type of inquiry which had to be undertaken in spare time and aside from official duties. Therefore I tried to confine investigation to those topics to which a contribution could conceivably be made, that is, to subjects that were not completely covered in the literature or about which the materials available are unclear, few in number, or contradictory in content.

One of the subjects which seemed worth persuing was the tremendously popular and important cult of the rice goddess, Inari, and the relations of this deity with the fox. The most sustained work on this subject is the monograph by D. C. Buchanan.[1] But Buchanan is concerned with origins rather than with description or a discussion of function. Consequently his volume becomes a dissertation on the origin of Shinto and of religion itself as much as an account of the worship of Inari. Besides, Buchanan's observations concerning Inari worship were made mostly at the great shrine at Kyoto. He therefore acquaints us with Inari worship in its official and most sophisticated form and with the State Shinto manifestations rather than with the cult Shinto forms which the people know best and practise most constantly.

Another scholarly work which contains useful information on the subject is by M. W. De Visser.[2] The approach of this writer, however, is primarily historical. He has assiduously combed the ancient chronicles of Japan for any reference to the fox. His interests are theoretical and he has a point to make. He is chiefly concerned with proving that the fox deity was the first goddess of rice and was displaced in time by Inari, whose messenger she then became. Consequently, De Visser, in spite of his exacting research and rich documentation, never gives us a picture of Inari worship in the Japan of his day. Perhaps the most adequate essay on the theme has been written by Lafcadio Hearn.[3] Yet this is a little too whimsical and literary in attack to be of greatest scientific value. More recently John F. Embree has contributed some valuable data in notes scattered throughout his general writings. However, he does not present

[1] Buchanan, 1935. (See bibliography at end of article.)
[2] Visser, 1908. [3] Hearn, 1901, pp. 310–342.

a systematic treatment of the Inari cult.[4] There are also references to the fox and Inari in Chamberlain,[5] Chamberlain and Mason,[6] and Mitford.[7]

In gathering material concerning the fox and Inari, I was most fortunate in having the intelligent and earnest help of one of the more mature Americans of Japanese ancestry, a well-educated young man who spoke excellent Japanese (he had lived in Japan for a short time) and who understood precisely the comparative and scientific nature of my interest. He had a large fund of information himself and, because of his knowledge of Japanese customs and his perfect command of the Japanese language, had access to the discussion groups and councils of the old people. His own family had been prominent in religious activities in Japan, and the older members of his household were therefore particularly well informed concerning the topic under consideration.

The account which follows was obtained in this way. One evening my friend told me what he knew about Inari and the fox. His absorbing narrative led to a number of questions, some of which he could not answer with certainty. After obtaining satisfactory replies to these queries and supplementing his knowledge by talks with his elders, this young man organized his thinking on the subject and drew up an outline of points to be covered. For the next two evenings he dictated his account to me and I took down his words verbatim. When the results were typed, we went over them together to add any details that might have been forgotten and to correct any mistakes that might have crept in. Naturally I hoped that this man would allow his name to be used in connection with this article, at least as co-author. However, so uneasy have Americans of Japanese ancestry been made by some persons who point to any knowledge of Japanese culture as an evidence of sympathy with the late enemy, that my helper has requested that his name be not used.

Inari is supposed to be a goddess[8] who came, according to myth (*shinwa*, "god story"), in the beginning during a famine at the time of the creation of Japan. She descended from Heaven riding on a white fox, and in her hand she carried sheaves of cereal or grain. *Ine*, the word now used for rice, is the name for this cereal. What she carried was not rice but some cereal that grows in swamps. According to legend, in ancient times Japan was water and swamp land.

In one way the worship of Inari is Shinto (i.e., State Shinto). Shrines in Japan are divided into three main groups, Empire sponsored (*kokuei*, "State managed"), ken sponsored (*kosha*, "old shrine"), and village sponsored (*sonsha*, "village shrine"). The highest of the four classes of shrines sponsored by the Empire is the *kampei taisha*

[4] Embree, 1939 and 1941. [5] Chamberlain, 1890, pp. 87–91.

[6] Chamberlain and Mason, 1901, pp. 49, 336–337.

[7] Mitford, 1901, pp. 187–188, 243, 255–260, 261–269.

[8] There is some dispute over the sex of Inari, though most authorities and most Japanese consider Inari (or the principal manifestation of Inari) to be a female. Actually at the main Inari shrine at Fushimi in the southern part of the city of Kyoto, nine deities, some female, some male, are enshrined as Inari. Buchanan and De Visser have extended discussions of this point.

which includes the shrines to emperors. The great Inari shrines belong to this class;[9] this shows that Inari is very important. Since Inari is a bearer of food she is more or less taken as a goddess of commerce.

But in another way Inari is more or less for the common people. The shrines to Inari are about the most numerous in Japan and every village has one. Some hamlets are so small, perhaps with only twelve houses or so, that they have no shrine keeper or shrine master, but there will be one person who cares for the shrine and sweeps the court (*keidai*) each day. If someone in the village gets in trouble with the fox he can go to an Inari shrine which has a shrine keeper, one perhaps just a few miles away.

Any master or keeper of a Shinto shrine is called *kannushi*, "shrine or palace master." The shrine master (priest) of an Inari shrine is called this too. The shrine master's position is hereditary and descends from father to son. The oldest son would take it if there is more than one son; if there is only a girl, she can marry a man who takes her father's name and carries on the shrine work. This has been done lately. In very ancient times it was not permitted, however, I understand.

According to the thinking of the people, there is a great deal of connection between Inari and the fox (*kitsune*). As I have explained, Inari is said to have come from heaven riding on a white fox. They say a fox does not get white until it is very old, until it is a thousand years old; such a fox is very wise. The fox on which Inari rode was probably a female. Most foxes are referred to as female.

The association between Inari and the fox is shown in many ways. Fox is said to be the messenger for Inari. Invariably you find foxes carved out of stone set before large Inari shrines, on both sides of the shrine, as guards. Often they have the right paw lifted in a sort of beckoning gesture. Inside the shrine there is usually a scroll which says, "Inari, great goddess" (*Inari dai myōjin*). But since fox is the handmaiden to Inari, the Japanese people, though they know Inari is the goddess, seem to think that the fox herself, though not a goddess, is quite powerful. In fact the fox is often taken to be synonymous with Inari. *Kitsune* is the Japanese word for fox, but often the fox is called O-Inari. The "O" is honorific.

The connection between Inari and the fox is shown in another way, too. Since Inari is a bearer of food and goddess of commerce, nearly all the Japanese shops have a miniature shrine of Inari right there in the place of business. In these shrines there is always an image of a squatting fox with the right paw lifted in that beckoning gesture.

Because Inari and the fox are so closely related in Japanese thinking, the shrine master of Inari has great control over foxes and takes a great interest in them. During the winter time when living is hard he would call the foxes and feed them. He calls them by name and goes out in the hills and feeds them, placing dishes of food, of soybean cake, out for them. Perhaps there are twenty foxes in the region, in the vicinity of the hills around the shrine. There is a head fox (*oyagitsune*, "parent fox") who is the oldest, the wisest, and the one with the best record. The foxes are not exactly deities and therefore they get into trouble. They have better or worse records. The shrine master knows all about them and can call the head fox or another fox. He can request a fox to stop troubling someone or can ask the help of the foxes in finding some lost object. The fox may not manifest herself, but when the shrine keeper goes into a trance the fox can

[9] On June 18, 1871, the Fushima Inari shrine was raised to the rank of *kampei taisha*.

show him where to find the lost object, the thief, or the lost horse. Also fox has the attribute of seeing what will happen in the future, and so many people, especially the farming people out in the country, will consult the fox through the Inari shrine keeper before any important event, such as going on a journey, or even when they want to get married.

Because the fox can see into the future and help people, those who planned to come to the United States from Japan used to buy a little image of a fox in a novelty shop and go with it to an Inari shrine. The shrine master mumbled some words over it and then it had power. The immigrants brought these with them. Very few people came to the United States without consulting an Inari shrine first to see whether it was going to be good luck.

One man brought a little image over to this country. He set it up and said it helped him and told him what he wanted to know. He used to get advice about gambling from it. Once he went into a Chinese gambling house and lost a lot of money, so he went home and scolded the fox image for not warning him or helping him. That night the fox came to him in a dream. She explained that she had not helped him because she had not been able to stand the smell of the joss sticks in the Chinese place. The Chinese burn these to keep out the evil spirits, and the fox said she has no power in these Chinese places.

When there are shrines to Inari or to fox, if a visitor brings candy or food, some of this is put in the shrine and is eaten later by the people. Putting it there is just a gesture; a food offering is common in any shrine.

As I have said, most foxes are thought of as females. Most foxes are called *sada-gitsune*. Sada is a female name; you find it in the common girl's name, Sadako. It emphasizes the feminine characteristic. It is a name something like Reynard the fox of the English-speaking people. It tells something about the characteristics of the animal. You can see that the fox is thought of as a woman because a man whom we would call a "sly fox" or "foxy" in English is not called that in Japanese, though a woman with such characteristics is called this. A man with such characteristics is called *tanuki*, "badger," or *furu tanuki*, "old badger."[10] A gold-digger or a sly woman is often called *megitsune*, "female fox." Sometimes *furu gitsune*, "old fox," is used of a troublesome woman.

Foxes are noted for their tricks. In the time when my folks were back in Japan, thirty-five or forty years ago, it was only a brave person who went out at night and traveled to a different village. This was because in travel at night the fox might try tricks on him, especially if he were carrying food and the food was oily.

Fox can assume almost any form, animate or inanimate. The special trick of the fox is to change into a beautiful maiden. The traveler is met by this beautiful girl who asks directions of him. When he gets home he finds that he has only rocks or something like that in his carrying cloth (*furoshiki*). He has lost his belongings. He doesn't know what happened but he remembers meeting this girl. Most meetings with the fox end in such tricks.

There is a way of counteracting this. If a person thinks he is meeting a fox in some other form, he should turn around and look at the being through his legs. If he sees a

[10] Badger, like fox, can deceive and possess human beings. Foxes and badgers are sometimes classed together under one term, *kori*, according to Visser.

fox, then it is really a fox. If it is a human being he will see a human being. People who were traveling used to do this.

Another way is to put saliva on the eyebrows. Then you can't be fooled. "Put saliva on the eyebrow" (*mayuge ni tsuba o tsuke*) is a command, even in colloquial expression. If you think someone is being fooled you say, "Put saliva on your eyebrows." Or if you realize that someone is trying to deceive you, you can say, "He was telling me this but I put saliva on my eyebrows."

There is another thing to do if you meet a fox. Call out the name of a god or goddess. Invoke the name of one you believe in. In Japan it would be the personal deity of the village where you were born.[11] These patron deities are called *ujigami* ("master deities"). Calling the name of the deity is done to help in any great danger. Some of the American-born, especially the Buddhists, do it even today. They will call the name of Buddha during a thunderstorm or when it lightens. This does not concern the fox particularly but it shows how strong the belief is and why it is used against the fox. Those who were going to come to America, before they started, used to get, at the village shrine, a piece of paper with the name of the village god on it. It is supposed to protect you wherever you are. I remember the case of a neighbor of ours (in America) whose boy got pneumonia; it turned to pleurisy and the boy went to the hospital. His mother, though she was an *issei* (alien, born in Japan), was young, about twenty-eight years old then. She got up and prayed to her village god every morning. She thinks that helped a lot, for the boy got well.

An especially mischievous fox will do things more serious than taking away food. The fox can make things appear to be what they are not. In the middle of a Japanese rice paddy usually there is fertilizer in liquid form in a barrel. Fox will make it appear to the traveler like a bathtub, and the next morning he will be found bathing in the filth. He will explain that he was invited into a beautiful home and after a delicious meal was invited to take a bath. Or he walks all night long in a circle in a rice paddy thinking he is going somewhere. When people are found in this condition the shrine master of Inari is called "to get the fox off him" (*kitsune wo noku*). This is why anyone who sees a fox gets worried. He thinks that perhaps he has met one of these mischievous ones.[12]

Foxes have weddings. No one has actually seen one of these weddings, but they say there is a wedding procession. At night people see a procession of paper lanterns in the

[11] Another way to expose a disguised fox, I was told, is to watch its shadow. The shadow of a fox which has assumed human form will retain the animal shape.

[12] A case in which his father feared he was being approached by a mischievous fox was related to me by an American of Japanese ancestry: "My father was pretty hard-headed. He didn't believe in this and that like some people. He had only one experience with the fox,—not with fox directly but with fox fire (*kitsune bi*). It was when he was a young man going to college. He was about nineteen years old. The college was nine miles from his home. This time he was walking home in the evening for the weekend. Quite a ways ahead of him he saw what looked like a paper lantern (*chōchin*) in the dark. He thought someone was traveling toward him. Then he lost sight of it. Pretty soon he saw it behind him. Then he saw it in front again. It kept on like this. It was never very close to him. He took this to be fox fire. He told me he had a notion that the fox was trying to fool (*damasu*) him. He thought of different things he would do if fox appeared before him, like knocking it out with a blow and running. But nothing happened to him. He was glad when he got to the place where he was going."

distance. Foxes' weddings were a very common occurrence in the old times. Children would peek out when lights were seen in the distance and say, "It is a fox's wedding." Even the elders would dash out to see. The people don't actually see the procession; they just see the lights. They are afraid to get close; they might get possessed (*nori utsuru*) by the foxes. There is another belief about foxes: the barking of the fox is considered to be bad luck. The sound is like sawing and is said to mean the sawing of wood for coffins.

But what people really fear about the fox is that it can take possession of a person's mind and body. People are very much afraid of this because they say that even if the fox is exorcised by the shrine master and the patient returns to normal, his life is shortened by what has happened. The experience takes away a certain amount of animation or life force and the person may die young, perhaps ten or fifteen years later, when he otherwise would have lived much longer. The Japanese have a phrase, *ki ga nukeru*, "vitality (spirit) leaks out," which is used about this. They also say *jumyo ga chijimu*, "his life is shortened."

Both men and women can be possessed. Usually it is a female fox that plays the dirty tricks. Of course this possession is usually carried on by a renegade fox. This kind of fox gets into the habit; once she does it, she keeps on possessing people.

Among the foxes a few may turn out to be renegades like this and are expelled from the court of foxes. They are usually banished because they took possession of a human body or mind for spite or for revenge or out of pure mischief. When the shrine master or keeper of the shrine calls the head fox for an accounting, if the reason the offending fox gives does not justify what she did, she is banished and becomes a renegade. A fox may be a renegade but the head fox and the shrine master still have great power over her. That is why the shrine master of Inari can help you if you are possessed by a fox.

When a person's mind and body are possessed by the fox, he acts as though he had amnesia. He doesn't quite know what is going on and is vague about things. He has a voracious appetite for *age* (fried soybean cake). Often *age* is referred to as Inari, because it is fox's food and the fox likes it so much. The patient has an enormous appetite for bean cakes, since that is the fox's favorite food. There have been cases where a man who is possessed by fox has eaten a bushel of these bean cakes although ordinarily they are hard to eat by themselves because they are oily.[13]

Or a person possessed by the fox may act like a fox. My father says he saw this and my grandfather saw it. This man who was possessed by a fox jumped over a *torii* (gateway before a Shinto shrine) which was about twelve feet high. He could do it while he was possessed because he had the ability of the fox to jump.

As soon as these signs are noticed, the Inari shrine master is called by relatives or friends to perform certain rituals such as waving *gohei* (sacred wooden wands with paper streamers attached), the symbol of a god's power, over the patient. He calls out certain incantations. He calls out to Inari and then goes into a trance.

[13] Another favorite food of the fox which those possessed by fox often crave is *inarizushi*, "inari packed." Bean curd is cooked in oil until it is *kitsuneiro* or "fox color," that is, yellowish. Then the square is cut into two triangular pieces. Rice and vegetables are cooked separately and then are mixed together with vinegar and sugar. The bean curd triangles are stuffed with this mixture.

When the shrine master is going into a trance to find out why a person is possessed or where some lost object is he sits in a folding chair (*shōgi*). In Japanese homes they don't have chairs. You usually find *shōgi* only in shrines and places like that. The shrine master sits in his chair. He gets pale. He doesn't talk in a normal voice. He goes into a semi-trance and asks the victim what is the reason for the possession. Then he goes into more of a trance, and the fox in the patient answers through the shrine master's voice and tells what the offense was. You can tell by his tone whether it is a male or a female fox that is causing the trouble. Usually it is a female fox. Sometimes the offense is a very little thing, like stepping on a fox hole or frightening the baby foxes away. Then the shrine master asks the fox to leave the body, and when it does usually the patient gives a loud cry and falls over. When he revives he is sane and normal again. He does not remember what has happened during the time he was possessed.

A fox can travel with the speed of thought. That is why the Inari shrine master is the one to go to about lost objects. To find it he sits in a *shōgi* in the same way and goes into a trance. First he goes into a semi-trance. He tells the fox to go out and find the missing object and says it will take a few minutes. He sends out the fox leader and other foxes. They can travel fast and go in all directions. Then he goes into more of a trance and tells what the fox says. The foxes find whatever is lost and report it to the shrine master. For instance, they tell that the lost horse is in such or such a place.

When a shrine master helps you, you give him a gift called *rohai*. What is given depends on a person's wealth and is usually in money. No sum is asked for; the shrine master would not do that.

Historically, there was only one time that the fox did anything important enough to be noticed nationally. In the old days every feudal lord had a fort and every fort had a five-storied tower. This particular lord, *Osaka jōshū*, "Lord of Osaka Castle," happened to have a tower built over a fox's home. Later, people claimed that a ghost lived up there. People who saw something in the tower went crazy and everyone was afraid to go there. Finally a Buddhist priest went up to find out what it was. He found a fox there. She was in the form of a beautiful woman and talked to him. She told him that the tower had been built over her home and that she had no place to go. A compromise was finally reached whereby a fox cairn (*kitsunetsuka*) was built nearby. There was no more trouble. This happened during the rule of the sixth or seventh Tokugawa Shogun, about 250 years ago. I once saw a Japanese movie in which this episode was shown. The movie was not on this theme, but it appeared as a side issue. It is a nationally known story. I've heard it from people of different ken.

In order to determine whether the beliefs and practices concerning Inari and the fox which immigrants describe for the Japan of their youth[14] exist today, I showed this account to a young friend, Robert Seido Hashima. Mr. Hashima, who lived in Japan from 1932 to 1940, assured me of the vitality of the Inari cult in present-day Japan and gave me the following remarkable story of fox possession and a cure involving trance which occurred in 1938.

[14] A law providing for the total exclusion of Japanese from the United States was passed by Congress in 1924. Consequently, the Japanese immigrants' memories are of Japan as it was before this date.

Shrines to Inari and fox are very common in Japan, in fact so common that worship of Inari is almost like a distinct religion or cult by itself, instead of being lumped together with various other kinds of shrines that are found in the towns and villages. To give you an idea of what the beliefs about Inari and the fox are and how the ceremonies for these two are carried out, I'll tell you a story about possession by fox.

This happened in a town in Hiroshima-ken about seven years ago. I was seventeen years old at the time. There were about seven villages and towns in the area where I lived. The girl who was possessed lived in the town next to ours. She was twenty-three years old and single when it happened. I didn't know her personally but I have seen her and know who she is. There was nothing peculiar about her. She was a girl of average appearance and average intelligence. When she was cured of fox possession it caused a big sensation. Everyone was talking about it. So I asked a man I knew who lived in this girl's town about it. I said to him, "I heard such and such. Do you know anything about it?" He was an older fellow, in his thirties when this happened. He told me, "Yes, I know all about it. I was right there and saw it myself." It turned out that his family lives close to the girl's family. When the girl was cured her relatives and the neighbors were invited to see the *kitsune harai*, "fox chasing away," performed by the *kannushi*. He saw the whole thing. People who are invited like this are supposed to help by hoping for a cure, by concentrating, and by never interfering with the *kannushi's* meditations. What he told me about the case, as I remember it, was this:

The family of this girl noticed that she was getting very absent-minded and dreamy. Others noticed it too. She couldn't remember things and acted as though someone were leading her around and directing her movements. In her village was an Inari shrine with a court. It was not a large shrine and it had no regular *kannushi* taking care of it. A carpenter and his family lived next to it and he kept the court cleaned up. This girl used to wander down to this Inari court and stay there. That's why they thought that her trouble had something to do with Inari or fox.

At another town there was a larger Inari shrine. Two *kannushi* took care of it. So these *kannushi* were asked to come and see what was the matter with this girl. They decided on a day that would be lucky. They always do that. On the day they had named they came to the patient's home. They were dressed in the white robes of Shinto priests and brought *gohei* (sacred staff with pendant paper cuttings) and the things they needed.

The principal priest (*jinjo*) knelt to the south of the room with his assistant beside him. His assistant was the one who moved around, who led the girl around and took care of everything. The girl knelt at the west while the family and friends were at the north. This all happened in the daytime.

First the *kannushi* did the usual things they do at a ceremony of almost any kind. They waved the *gohei* to drive away evil and chanted a long prayer or *norito*. Then the head *kannushi* began to meditate. He sat there for a long time. He began to go into a trance (*gyo*). The people recognized this because he sat motionless, with not a muscle moving. He grew pale. When he spoke it was not in his natural voice. His lips scarcely moved when he talked and his eyes were closed.[15]

[15] In his study of the Japanese of Kona, Hawaii, John Embree has an excellent account of a state of trance and fox possession into which a priest of Inari went. See pp. 154–157 of this work.

He asked the girl questions, but it was the fox in her that answered. He asked the fox if it had taken possession of her and why. The fox replied through the girl but everyone knew it was not the girl speaking. The voice was much higher pitched and stronger than hers. Fox said that she had possessed the girl because she had broken off a branch of a tree at the Inari shrine and had thrown it away. Fox said that if it had been a little child who had done this she would not have punished the person in this way, but that a grown girl should have known better.

The *kannushi* tried to persuade the fox to leave the girl, but the fox refused. Every once in a while the girl would try to go out of the room. The fox was making her do this. The assistant would keep her there. Finally the assistant *kannushi* took a small piece of rice paper, crumpled it up, and put it right on the surface of the mat in the center of the room. He then lit a piece of rope made of twisted white rice paper (*koyori*) from a candle and ignited the rice paper with this.

Now something wonderful happened. A great fire that filled the center of the room blazed up from that little piece of paper. It reached to the ceiling and came within a foot or so of the people along the side of the room. But nothing was burned.

Then the assistant led the girl to the southwest corner and tried to make her walk through the fire diagonally across the room. The fox that was in her tried to keep her from doing it and said, "No, don't do it! Don't do it!" The girl struggled and it took the assistant a long time to force her through. But he struggled with her and finally got her to the fire. As soon as she got into the fire and was in about the center of the room she gave a cry "*kwei*," and fainted. It was a cry such as an animal gives when it is shot. It was the fox leaving her body. At the moment she fainted the *kannushi* recovered from his trance. And as soon as she fainted the flames died down and there was nothing but some ashes in the middle of the room. The spirit of the fox had left its own body and had gone into this girl's body. With that cry the spirit of the fox was driven from the girl's body again. Maybe it got back to its own body, but the fox probably died after this. The whole ceremony took about two hours.

The girl was put to one side of the room in a cool place. It took her three hours to come back to her senses. After she recovered, the *kannushi* asked her if she remembered what had taken place in the ceremony. She didn't know about any of the things that had happened. He asked her if she remembered cutting a branch at the court of the Inari shrine. She remembered doing this and explained that the branch was sticking out into the court from a tree which stood nearby. She thought that someone would run into it and hurt himself, so she cut it off. The *kannushi* said that her intentions were all right but that there was a proper way to do these things, and the next time she wished to change something at an Inari shrine she had better consult a *kannushi* of Inari first.

The family of the girl gave some pay to the *kannushi*, whatever they thought right. The girl received a piece of paper from the *kannushi* which had on it the words *Inari dai myōjin*, "Inari, great goddess." She put this up in her home. After this the girl took a great interest in Inari. She prayed often at Inari shrines and gave offerings there.

The girl was back in her right mind as soon as she recovered from the fainting. But she was physically weak for three weeks after that. She seemed to be quite normal from then on and not long afterward she married.

I told my friend that it was hard to believe what he told me about the great fire and

how it died down so suddenly. He said that he also had not believed such things before but that he had been present and seen it with his own eyes. He said it was like a miracle performed before him and he could not deny it. He said that everyone present had seen the same thing and had been agreed on what he saw.

Mr. Hashima also confirmed a point made by Buchanan and others, that the worship of Inari and fox, because it is so popular, often is carried on at places primarily devoted to other functions. To make this clear he told of a spot in Hiroshima-ken, not far from his home, where the main attraction is a Buddhist temple, but where a hut at which offerings to Inari can be made is also found. A tree around which a sinister legend has grown up is also located here. His description of the place is no less notable for what it reveals about a form of black magic than for what it adds to the study of the Inari-fox complex:

Sometimes there are places where Inari or fox can be worshipped which are not regular Inari shrines but are near a Buddhist temple. At a town near the one where I lived in Hiroshima-ken there was a place like this near a Buddhist temple of the Jōdōshu (jōdo sect). I just went there at the time of their festival so I don't know so much about it. The place is famous for a cryptomeria tree (*sugi no ki*) about which there is a story well known in the vicinity.

A samurai was betrayed and killed by another man. His wife, being a woman, was weak and couldn't take revenge directly, but she swore to avenge her husband's death. She practised a ceremony of *noroi kugi* ("hate nail") against her enemy. She went to a blacksmith and asked him to make her five nails. This is a dangerous thing for a blacksmith to do because he says a *norito* (prayer) over everything he makes and if something goes wrong with the ceremony it comes back on him and kills him. You would have to convince a blacksmith that he should do a thing like this. It's secret.

When this woman had the nails she painted the outline of her enemy on white paper with *sumi* (black ink). Then she pinned the paper on this tree. She stood about fifteen feet away and threw the five nails at the figure. She vowed to do this for one hundred days. You have to keep a vow like this. If she had done it for only ninety-five days it would have come back on her and she would have died. The more times you hit the black image the worse it is for the victim. The woman kept it up for the 100 days. She prayed each time that she would accomplish her purpose. The man who had betrayed her husband sickened. He grew very restless. Though he ate his body wasn't nourished. He finally died in great pain. The marks on the tree where the nails hit show even today. Nails used like this are a little longer than ordinary ones. They have to be made especially for the purpose; any nails won't do. It is women who are described as carrying on *noroi kugi*. Men can settle things out in the open, with weapons.

Every once in a while you hear of a case like this. Not long ago some people were saying this about a woman who lived in our village. They didn't accuse her openly; someone would just whisper it to someone else when she passed. I just took it as gossip. I didn't know whether there was anything to it. Her husband died in terrible agony all right and she had not been getting along with him. There was some other man mixed up in it. So some thought that she had used *noroi kugi*.

But to get back to the Inari place near the tree and the Buddhist temple. I don't

know whether the tree or the temple and this Inari place was there first. It's a hut (*koya*) with the lower part of stone. In this lower part there is only one small opening in the stone. On the ledge of this opening people put soybean cakes as an offering. No one sees the bean cakes being taken, but they always disappear. They say there is a cave under the hut and that a fox who eats them lives there. So those who want Inari or the fox to help them go to this place and pray and leave food offerings in the hole.

WASHINGTON, D. C.

BIBLIOGRAPHY

BUCHANAN, D. C.

1935 *Inari: Its Origin, Development, and Nature.* Transactions of the Asiatic Society of Japan, Ser. 2, 12, pp. 1–191.

CHAMBERLAIN, BASIL HALL

1890 *Things Japanese.* London and Tokyo.

CHAMBERLAIN, BASIL HALL, AND W. B. MASON

1901 *A Handbook for Travellers in Japan.* 6th ed., rev. John Murray, London.

EMBREE, JOHN F.

1939 *Suye Mura: A Japanese Village.* The University of Chicago Press, Chicago.

1941 *Acculturation Among the Japanese of Kona, Hawaii.* American Anthropologist Memoir, 59, pp. 1–162.

HEARN, LAFCADIO

1901 *"Kitsume." Glimpses of Unfamiliar Japan.* Vol. I. Boston.

VISSER, M. W. DE

1908 *The Fox and Badger in Japanese Folklore.* Transactions of the Asiatic Society of Japan, 36, pt. 3, pp. 1–159. Yokohama.

THE FIRE-DRIVE AND THE EXTINCTION OF THE TERMINAL PLEISTOCENE FAUNA

By LOREN C. EISELEY

I

THE subject of extinction has a perennial, an almost morbid fascination. The total disappearance of a species, of a unique and never-to-be-duplicated genetic pattern, has in it, moreover, something of the mystery that lingers about the origins of those same patterns. And when, in addition, that disappearance is found to be correlated in time with the first human intrusions into the New World, then, indeed, our speculations multiply. The adage "let there be some uncertainty about your departure" is sure advice for the creation of that romantic aura which lingers about the unknown and the unexplainable. The terminal Pleistocene fauna took just that mode of exit from the stage of life. As a result its scientific immortality is certain, not alone among paleontologists, but among students of mankind as well.

Yet fascinating though the subject of extinction is, it is perhaps significant that—so far as I am aware—there exists no volume in English devoted to its elucidation. There is a simple reason for this. Theories are many, but most unprovable. Or pertinent objections to their general usefulness can be raised, even if we grant their applicability in particular instances. It is this situation that causes the biologist to despair as he surveys the extinction of so many species and genera in the closing Pleistocene. Are there many particularistic explanations chancing to correlate roughly in time? Or did the climatic events attendant on the glacial retreat in some manner affect animals of quite diverse habits and physical structure? In beginning a survey of this subject, one can not do better than to peruse Henry Fairfield Osborn's now almost forgotten paper, "The Causes of Extinction of Mammalia," which appeared in 1906.[1] In addition to valuable data, there appear in this paper certain theories which, interestingly enough, have been repeated in later times by writers deeming themselves original. Actually only two new theories have added anything of a very startling nature to the information assembled by Osborn nearly forty years ago. And even in one of these instances, Osborn had already projected—in the less exact terminology of that time—something of the underlying idea of the Sewall Wright hypothesis.

One fact which constantly confronted Osborn in his treatment of extinction was the extent to which natural disasters in the shape of epidemic disease or climatic catastrophe are sufficient to explain an enormous reduction in the numbers of a particular species, but are yet inadequate to illuminate the reason for the inability of the species to rebound, in a few years, from its decimated condition. He comments on the fatal influence of inbreeding, noticeable in some

[1] Osborn, 1906. (See bibliography at end of article.)

instances, on much diminished herds such as those of the protected European bison.

To-day, according to Sewall Wright and his co-workers, such an initial restriction in the size of a given population would, first of all, greatly reduce the number of breeding animals. This, in turn, would lead to a reduction in the amount of hereditary variability present in the stock, increasing homozygosity and a more or less random fixation of genes without reference to their adaptive value. Such circumstances in an already numerically circumscribed group may accelerate the trend toward extinction. It has been noted that isolated islands are, in a sense, "evolutionary traps," that, by the process described, often bring about extinction because of the increased genetic uniformity in a small population. Admittedly, of course, the extinction of certain of the widespread Pleistocene animals cannot be viewed in exactly the same light as disappearances in island "traps." Even if we grant the validity of the Sewall Wright theory, something—some other potent factor—would have had first to decimate the Pleistocene fauna before gene fixation could get in its work. In fact, in Simpson's eyes, number has to be so reduced before the Sewall Wright effect can function that its importance in relation to evolution has probably been overemphasized.[2]

The second of these newer theories—one brought forward recently by Dr. Carl Sauer—is of peculiar interest to the archeologist. In the course of an extensive summary of late discoveries connected with human intrusion into the New World and the relation of the early comers to the geographical environment in which they found themselves, Dr. Sauer has advanced the hypothesis that the terminal Pleistocene fauna was destroyed by hunters making widespread use of fire-drives in the pursuit of game.[3] This weapon, he argues, would have been far more effective, in terms of mass destruction, than the lance or atlatl. Great body size in itself would have made the Pleistocene forms easy victims to wind-driven fires, since the huge animals would have been lumbering and unwieldly runners. Sauer deems it probable, furthermore, that the great grasslands are of relatively recent origin, and that man-made fires have played a major part since prehistoric times in the creation of circumstances favoring the dissemination and evolution of the grasses. This process, he suspects, began with the intrusion of man into the New World during the closing Pleistocene, say fifteen to twenty thousand years ago. Dr. Sauer does not clearly limit his comments to the American scene, but suggests a somewhat similar type of extermination in the Old World. It is Man, not the climatic shifts of the Pleistocene, he feels, that destroyed this great fauna.

In presenting these provocative views, Dr. Sauer is pioneering in one of the most fascinating fields of scientific speculation. Archeology is indebted to him for this contribution. Only by the constant testing and discarding of theories, the propounding of better hypotheses based on what has survived this unceas-

[2] Simpson, 1944, p. 67. [3] Sauer, 1944.

ing refinement of raw data, can advancement ultimately be made. Dr. Sauer's theory may very well have merit as an explanation of certain Pleistocene events. At the same time a close appraisal of it reveals the need for a certain careful and objective analysis. With the theory of grassland origins we shall not attempt to deal here, primarily because it is not highly germane to the problem of extinction and because it would, in addition, demand a wide excursion into botany.[4] Instead, we shall be at pains to examine point by point Sauer's contentions in regard to the fire-drive. It will be apparent at the close of this survey—or so the writer believes—that Dr. Sauer's thesis, interesting as it is, does not satisfactorily answer a good many questions about Pleistocene extinction. But, in that respect, Dr. Sauer is none the worse off than the rest of us, who must continue to be baffled by the mysteries of extinction. He, at least, has contributed another item to Osborn's list of long ago.

II

Probably the easiest manner of approaching Dr. Sauer's paper is to take his major contentions, italicize them and then append such material as seems pertinent to their discussion. Let it be emphasized that these data will not be used in a mood of outright refutation, but only to make clear the difficulties of accepting *any* single and exclusive view of Pleistocene extinction.

1. *It was the big and clumsy animals that disappeared and the new element added was hunting man.*

Most certainly a large and spectacular fauna disappeared, but it was not all big, clumsy, or gregarious. Besides certain molluscs, *Bufo nestor*, an extinct toad; the Archaeolaginae, a sub-family of rabbits; the dire wolf Aenocyon; Capromeryx, Tetrameryx and Stockoceros, three forms of swift antelope; the sabre-tooth Smilodon; Arctodus, the short-faced bear; and, among others, *Equus tau*, a small horse, disappeared. A full coverage of the literature would add other items to this list. Grant that man was interested in parts of this fauna. Nevertheless a number of these forms were swift-moving or solitary and not easily cornered — even with fire. Moreover, of all forms least apt to be seriously threatened by primitive man, birds are preeminent. Fires do not destroy a bird fauna capable of flight. Yet amidst this terminal Pleistocene assemblage, several birds became extinct. It must also be taken into account that, up until quite recently, all of the great American museums have concentrated to a very considerable extent upon the collection of the large and spectacular fauna of the Pleistocene. Small forms have been neglected; in fact it is only recently that techniques have been devised for obtaining them. Hence the extinction of the large forms has taken on a somewhat exaggerated importance. Our knowledge of birds and of other small fauna is not nearly so extensive.[5]

[4] It is to be noted, however, that Clements and Chaney do not accept this view of grassland origins. See pp. 33–34 of their monograph.

[5] "The reconstruction of the past mammalian life has until recently been based almost en-

2. *The fire-drive was a basic hunting practice among modern Indians. It is a reasonable assumption that it was introduced by the early comers and that its effects were catastrophic.*

To survey the literature dealing with the hunting practices of the Plains peoples would lead far afield and need not be attempted here. Let us accept the fact that fire was indeed used. Yet we still encounter the same problem which the present writer pointed out in connection with assumptions that the Folsom people exterminated their game by the lance or atlatl. Why, if this method was so deadly, did the living bison and the living antelope roam the plains in countless numbers? It is known that both these animals perished in occasional fires and floods, but the great life-stream flowed on in vigor until the riflemen came. It seems difficult to accept the destruction postulated for a scant number of early hunters and then to find that the same methods were having no observable effect on bison or antelope numbers within historic time.[6]

3. *Archeologic proof is difficult to establish, but nothing else is known that would explain the high frequency in these campsites of the large, powerful and protected mammals.*

We must note several things here. In the first place large animals are always preferred for food. The meat is generally better, and a single kill is sufficient to nourish several people for days, whereas small animals have to be pursued in numbers and with persistence. Big game, therefore, is always sought in preference to small forms. Bison were the most numerous of the Plains fauna and their attraction is clear. It is to be noted, however, that horse, camel and antelope—in no wise formidable animals—seem to be the forms next most enumerated from Folsom sites. Evidences of elephant association are far less numerous, and in the Plains region, to date, there are no evidences of great fire-drives in connection with mammoth killing, no traces of great herds driven over the rim rock of valleys, no evidence of the piling up of numerous individuals, no remains of the scorched bones of victims. Folsom man knew the mammoth, but to date there is little evidence that he hunted him with the same assiduity with which he pursued the bison and certain less formidable beasts.

This is not to say that the Folsom people feared the mammoth or could not hunt him—there is clear evidence of kills. But there exists a very genuine possibility, only to be clarified by further work, that the great beast may already have been less numerous than his confreres. However this may be, there

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tirely on the larger and more conspicuous forms. Only occasionally have the remains of the smaller mammals been taken in association with the larger species. In fact, it was believed for years that the remains of the smaller vertebrates were mostly missing from the deposits. The demand for spectacular exhibits has led each field party to search for larger and better specimens, constantly overlooking, without a doubt, deposits containing small vertebrates." (Hibbard, 1941.)

[6] The bison held their own even after the domestication of the horse and the introduction of fire arms. The destruction of the herds actually began only with the advent of the railroads and the robe hunter (Clements and Chaney, 1936, p. 33).

is absolutely no trace of his extermination in anything like the catastrophic numbers demanded by the theory of the fire-drive. The bison, by contrast, obviously was trapped in drives of some nature, but this method employed on the existing bison still did not lead to its disappearance in spite of vast kills. Moreover, the Folsom point, whether used with lance or atlatl, was quite capable of killing bison. Instance the point found at the Lindenmeier site, driven between the vertebrae of *Bison taylori*. These weapons were not ineffectual even if employed cunningly from ambush or in the midst of a stampede over gullies or rim rock. As for horse, camel, and antelope, here again there is no evidence of mass kills.

Obviously we need more extended data from such vast camping sites as Lindenmeier, but the amazing thing about this particular site is the paucity of any elephant bones (confined, I believe, to just one fragment at the present time) whereas in Europe mammoth bones were much in evidence in the late paleolithic sites, being utilized, apparently, for a variety of purposes. Whether Lindenmeier eventually proves to be an exception in this regard remains, of course, to be seen.

4. *Predators, such as wolves, should have increased in response to the increase in numbers of injured herd animals, thus tending to destroy them.*

Interestingly enough, Smilodon, *Felis atrox*, and the dire wolf vanish with the fauna upon which they preyed. It is, of course, reasonable to assume that Smilodon, a highly specialized beast, passed with the elephants he hunted. But why, so long as bison and antelope continued to exist, did such forms as Aenocyon and the Arctotherine bears, to mention just two forms, fade out of existence? Presumably there was still game in plenty. Nor were they reachable by fire.

5. *The bisons and the mammoths ranged pretty well across the continent but made their principal domain the Plains.*

Though an exhaustive study of this problem would demand a careful differentiation of the late Pleistocene species of mammoths and the ecological distinctions among them, we may make one brief observation. The eastern mastodon was a forest dweller. He was not, typically, a part of the Plains fauna. He was not concentrated in the Plains that the Folsom people frequented. He was not susceptible to mass attack by fire. Yet he, too, perished along with the eastern forest sloths and *Castoroides ohioensis*, the giant beaver. Nor does the fire-drive adequately explain what transpired in the bone-strewn wastes of Siberia, Alaska, or certain of the Arctic islands.

Even the charge that the Folsom weapons would not have sufficed for large beasts of thick hide and fur overlooks the fact that the Eskimo used to attack the great sea mammals of the Arctic—whale and walrus—with no more elaborate equipment.

III

This survey of Dr. Sauer's main contentions reveals, I believe, the impossibility of using any one specific explanation to dispose of so complicated an

array of events, either here or in the Old World. It is quite probable that man played some small part in the final drama. But something which we may suspect as being at least partially linked with the mysterious climatic changes of the closing Pleistocene had already started that rich fauna down the road to disappearance. Sauer has doubted the possibility of epidemic disease striking across so many genera of animals. We may, with restrictions, agree with the evidence that Osborn has marshalled-evidence that certain diseases *do* strike through several forms as taxonomically remote from each other as camels and elephants. Furthermore, the humidity of the period of ice retreat might have encouraged the spread of such diseases. Such items may have contributed here and there to the final result. Nevertheless we may agree with Sauer that it is quite unlikely that epidemics alone produced the final disappearance of all the Pleistocene fauna. Forty years ago, after an exhaustive study of extinction, Osborn could only write: "Following the diminution in number which may arise from a chief or original cause, various other causes conspire or are cumulative in effect. From weakening its hold upon life at one point an animal is endangered at many other points."

We may, if we wish, attribute the failure to make a comeback to the Sewall Wright hypothesis of fixation under conditions which demanded a quick efflorescence of mutations which, in many instances, could not longer be made. We may note that some forms were adjusted to extreme cold, not to heat, or we may just as readily give some part at the close to man. But all of the amassed data, the strange disappearances in parts of the world where man had little or no foothold, argue against the reality of any simplistic explanation. Dr. Sauer's contribution is apt and impressive, but as a treatment of Pleistocene events, it raises as many problems as it seeks to lay. It would be better added to that list of cumulative factors proposed by Osborn which make toward the death of species, but taken alone are seen not to be sufficiently embracing to encompass the destruction of an entire fauna. The Pleistocene still keeps its mystery, probably because it has no single secret to lose.

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BIBLIOGRAPHY

CLEMENTS, F. E., AND R. W. CHANEY
1936 *Environment and Life in the Great Plains*. Carnegie Institution of Washington, Supplementary Publication No. 24.

HIBBARD, CLAUDE W.
1941 *Paleoecology and Correlation of the Rexroad Fauna from the Upper Pliocene of Southwestern Kansas, as Indicated by Mammals*. The University of Kansas Science Bulletin, 27, No. 6, pp. 79–80.

OSBORN, HENRY FAIRFIELD
1906 *The Causes of Extinction of Mammalia*. American Naturalist, 40, pp. 769–795; 829–859.

SAUER, CARL O.
1944 *A Geographic Sketch of Early Man in America*. Geographic Review, 34, pp. 529–573.

SIMPSON, G. G.
1944 *Tempo and Mode in Evolution*. Columbia University Press, New York.

NOTES ON CHEYENNE CHILD LIFE*

By SISTER M. INEZ HILGER

PRENATAL LIFE

AN expectant Cheyenne mother was not to eat the entrails of buffalo or, in more recent times, those of cattle. Doing so caused the umbilical cord to bunch and the placenta to adhere. No food restrictions were placed on the husband, nor on his nor his wife's relatives, during the pregnancy of his wife.

The umbilical cord was thought to wind around the body or the neck of the fetus if the pregnant woman wore a belt, did bead work, saddled a horse, used rope or cord for tying purposes, or turned in bed without rising to a sitting position or at least pivoting herself on her elbow. She was to turn often when lying down, for "this kept the baby loosened." Lying down too long at any time caused the placenta to adhere. Standing near a fire or poking up hot coals before delivery did the same; doing so after delivery caused a fever.

A pregnant woman was to walk around before sunrise, the belief being that the fetus did its growing then and that moving about "helped the child to catch up with its growth." After sunrise the woman might return to bed for more sleep and rest; however, only women who were not well were known to do so. In old days most women were up before sunrise and worked all day.

Among the Northern Cheyenne a woman might expect her child when born to resemble in appearance a person at whom she had stared during pregnancy. But the fetus was in no way affected if she looked at a corpse or was frightened by an animal. Southern informants, however, had been told never to be frightened by any animal while pregnant, not even a mouse or a grasshopper, nor were they to look at a deformed, dying, or dead person or animal. In fact a pregnant woman was not to let her eyes rest on any unusual object.[1]

All informants but one agreed that the sex of a fetus could not be predicted. A Southern informant had been told that a pregnant woman could predict the sex of her child by the manner in which a leg of veal cracked while she was frying it. "If it cracked lengthwise, the woman was carrying a girl; if crosswise,

* The Northern Cheyenne live on the Tongue River Reservation in Montana, the Southern Cheyenne on the Cheyenne-Arapaho Reservation in Oklahoma. The customs, beliefs, and traditions recorded in this article were related to me during personal interviews in the field by Josephine Wolf Tooth, Mrs. Brown Bird, Charlie Little Eagle, Thelma Two Two, Old Two Feathers, and Jane Shavehead of the Northern group in 1936, and by Mary Inkanish and Gertrude Lump Mouth Black of the Southern group in 1935 and 1940. I am not certain that I have enough material to infer that all traits mentioned in the article are institutional. I am grateful to the informants. To their information I have added a few notes of my own observation.

[1] For customs of the Chippewa Indians related to prenatal life and to other items found in the present paper see Hilger, 1936, and her *Chippewa Child Life*, forthcoming publication of the Bureau of American Ethnology, Smithsonian Institution, Washington. The customs of the Arapaho Indians are found in her *Arapaho Child Life*, also a forthcoming publication of the Bureau of American Ethnology.

a boy. It always came true for me. My chum did not believe it, but she tried it. Hers cracked lengthwise and later, when she gave birth to a girl, she knew it was true for her also."

Twins were predicted "if the woman's abdomen did not fill in below the sternum but did fill out well on both sides, or if a depressed line was formed from her navel downward when she was lying down. In fact if she ate both of the tenderloins of buffalo or cattle, especially of calf, she knew she would conceive twins." Prediction of twins by hiccoughing of two fetuses was not known to the Cheyenne.

A child was a reincarnation if it was born with teeth, with a patch of grey hair, with scars on its ears, or freaks in its anatomy, such as polydactylism, or syndactylism. "It was an old timer come back to life." "They were old Indians come back to life. If the child was a boy, he had been an old man previously; if a girl, an old woman." Birth marks were not significant. The reborn were not treated differently from other children.

In warpath days male babies were preferred "since they were more useful." In more recent days couples still hoped that their firstborn would be a boy, "but after that both boys and girls were welcome."

Sterility occurred. Its cause was not known. No blame was attached to childless couples. "Such couples felt badly because they had no children. No one else thought anything about it." "I never heard anyone cast reflections on childless couples. They had no children; that was all."

Abortions were rare. They were looked at askance and punished. "A miscarriage was considered bad. Such mothers were cared for but they were thought of as having committed murder. I have heard old women say, 'God knows you did that. And you'll get your punishment!' They were believed to have pressed their bodies against something, or to have eaten or drunk something to cause a miscarriage." "In the old days one of the four bands of men —they were called Fox Warriors, Crazy Dog Warriors, Red Shield Warriors, and a fourth group whose name I cannot recall—whipped a woman who had had a miscarriage. It was considered the same as having killed a baby. Willows were used in the switching."[2]

BIRTH

Among the Southern Cheyenne the child was usually born in a small tipi erected for this purpose near the home tipi by the woman's mother. If the woman's mother was not living, an aunt or another older woman erected it.

[2] The bands, generally called soldier bands, were institutional for the Plains area. One of their responsibilities was to perform police duties when called upon to do so by the governing body, the chiefs and headmen. Grinnell lists seven bands for the Cheyenne: Kit Fox Men, also called Fox Soldiers; Elk Horn Scrapers, also called Elk Soldiers or possibly, Fingers; Dog Men; Crazy Dogs; Red Shields, also called Bull Soldiers; Bow Strings; and Chief Soldiers. (Cf. Grinnell, 1923, 2: 48. This and Mooney, 1905–07, contain some excellent material on Cheyenne child life.)

Among the Northern Cheyenne the child was just as often born in the home tipi.

When delivering, the woman knelt on hay which had been placed on a buffalo hide. She braced herself by clutching a bar that had been securely attached, in a horizontal position, to two vertical poles. The vertical poles were about three feet apart and were firmly planted in the ground. The proper place for the brace was at the head end of the woman's bed. One midwife set two vertical poles into the ground, some distance apart, and to these she attached two bars horizontally. Then in a sitting position, the midwife braced her own back against the horizontal bars and placed her arms around the delivering woman's waist, pulling the woman toward herself. At the same time the delivering woman, kneeling before the midwife, pulled at a pole with tightly clutched fists. The pole had been firmly planted in an erect position just within reach of the woman. A chair was not used as a brace as was done by the Chippewa in recent times.

Any number of women might be present at a birth but always among them were older women. Persons who were unfailingly present were the woman's mother, some of her older women relatives, and midwives, that is, "some women who knew how to care for such cases." No man except a medicine man was ever present. The medicine man "sat by himself" a short distance away from the woman, shook his rattle and sang an incantation. At intervals he chewed herbs—these were "medicines"—and spat on the ground in the direction which the woman faced.[3] "Spitting in this direction will cause the baby to move toward the ground. All that helps to bring the child. The song helps too, but the rattling is merely to distract the woman from her labor pains. Sometimes he blows his breath on the sides of the woman's body." At intervals the woman was given an herbal decoction brewed by the women in attendance. A recently born animal, like a skunk, that gives birth to its young easily, was never dropped down at the neck between the clothes and the skin of a woman who was soon to be delivered.

After the delivery one of the attending women placed her first two fingers close to the navel of the baby, wound the navel cord twice around them and noted this length. Then one of the other women held the cord taut while the one who had done the measuring severed it with a knife.[4] A piece of clean cloth, in size about equal to the open end of a cup, was cut along a radius. Then the cloth was well saturated with grease and the radial cut slipped around the cord, and the cloth placed close to the body. The cord was then brought over the edge of the cloth opposite the radial cut, was again brought through the

[3] "Medicine" is a term used to designate magical power and should be differentiated from the term used to indicate an agent used in the treatment of a pathological condition.

[4] Grinnell says that the cord was wrapped once about the finger and then cut with a knife, in old times with a flint or arrowhead (1923, 1: 148).

center of the disk of cloth from the bottom upward and the open end of the cord placed on the cloth so that it pointed toward the face of the baby. This arrangement prevented blood from flowing. The cord and cloth were then again well greased, and after that a goodly amount of the ripe spores of the star puffball (*Lycoperdaceae*) was dusted over all.[5] One woman did not scatter the spores—"that was our talcum powder and was precious"—but incised the puffball and placed the open end on navel and cloth. A band of cloth was next tied over the navel and around the baby's waist several times. The two ends were loosely tied in front. The band of cloth was removed occasionally so that more of the puffball powder could be applied. It was permanently removed and the powdering discontinued when the cord fell off. Until the cord fell off, the mother of the child was not to sew with a needle nor to cut anything with a knife or scissors.

Immediately after the birth one of the assisting women either poked her finger down the woman's throat or tickled her throat with a feather. The gagging forced the ejection of the placenta. The latter was securely wrapped up and then tied into the crotch of a tree, some distance from habitations, high enough so dogs could not reach it. "It was treated with reverence since it was considered part of the child's body." "It is looked upon as the child's neighbor." The placenta was never burnt. Southern women said it was never buried, since this would cause the child's death; Northern women knew it was buried on occasions. The buffalo robe on which the mother had knelt, they said, was folded together so as to enclose the hay and secundines, and all was buried.

After the navel cord was severed, the medicine man who had assisted with his incantations and "medicines" washed the baby in an herbal decoction. After the bath the baby's body was greased to prevent chafing, especially its armpits, groins, and body from waistline to knees. The ointment used in greasing was made of powdered ochre (hematite) mixed with buffalo tallow. The ochre was gathered by women in certain rock formations and was considered "spirit paint." After the greasing the baby was laid on a softly tanned hide of young buffalo. Then a thick spread of finely powdered buffalo manure or powdered decayed pulp of cottonwood (*Populus balsamifera*) was placed under its buttocks, between its legs, and in its groins. This served as a diaper. Only thoroughly dried and aged manure was used. It was pounded into powder with a stone and pulverized by being rubbed between the palms of the hands. The cottonwood pulp was treated in the same way. Both manure and pulp were sifted through the fingers to eliminate little sticks or foreign substances. Collecting the material for and preparing the ointment, the "diapers," and "talcum powder" was done by relatives of an expectant woman, usually her own mother. Relatives, too, generally tanned the hide to be used as the baby's first clothing and made the baby's cradle.

[5] Cf. Grinnell, *ibid.*, for a similar note.

The father was never present at the birth of his child. He was outside the tipi assisting the woman who was preparing a meal for the attending women and the medicine man. He fetched water, replenished the fire, and did errands for the woman. He could do nothing in a magical way to facilitate the birth, such as pacing around. He was notified by one of the attending women just as soon as the child was born and was also told its sex. Thereupon, in old days, he fired a gun. After the baby had been bathed and the placenta and the buffalo robe on which the woman had knelt had been removed, he was invited into the tipi to see the baby and its mother.

For four days following birth the mother refrained from work and sat on the floor with legs outstretched and back against some stout support, such as a wall. For an abdominal support she wore a wide belt. Often she inserted a moccasin under the belt on each side as an additional support.

Mothers sometimes died at childbirth. Occasionally there were stillbirths. If a dead fetus was not delivered, a woman who knew how to assist in such cases removed the fetus from the uterus with her hands.

No significance was attached to the caul. "My daughter was born with a skin over her face but we made nothing of it. We never saved those skins." Birthmarks were treated with red ochre mixed with buffalo tallow. "Mary was born with a red ring on her cheek right near her nose. An old woman painted it with 'spirit paint.' It practically cleared up the birthmark. The old lady said 'There now! That cleared up Mary's complexion!' "

The child's ears were pierced with the point of a knife on the day of birth. Usually the father did the piercing. Before doing so he gave a horse to his wife's relatives. Additional holes were often pierced at the time of the Sun Dance, the tribal religious ceremonial. In the old days many Cheyenne had three or four holes, one above the other, along the edge of the ear.[6] Formerly the stem of a weed or a wisp of hay was inserted into each incision to keep it from growing together. Sometimes splinters of water-logged wood were shaven smooth, well greased and inserted into the incisions. In more recent times, among the

[6] De Smet, a witness to a Cheyenne ear piercing, wrote in 1852: "Among the Sheyennes the mother chooses the operator, and puts the knife into his hands. She extends the child on the skin of some animal, carefully prepared and painted, and which the Canadians call pare fleche. While one of the relations or friends holds the infant in a quiet posture, the operator makes five incisions in the rim of each ear. These incisions are destined to receive and carry ornaments. The mother makes a present of a horse to the operator, and another present to each one of the assistants. (Cf. 1881, p. 105.) Grinnell writes that a Cheyenne baby's ears were pierced when it was from three to six months old at a gathering of the people. The mother took the child to the place of meeting where the father requested the official crier to call out the name of a certain person to come and pierce the child's ears. The person, after reciting a coup, might then actually pierce the ears or merely go through the motion of doing so. If he pierced the ears, the father gave him one to three horses and perhaps other presents. If he did not pierce the ears, they were pierced later by any one and without further ceremony (1923, 1: 105).

Northern group, earrings made from bullets hammered into circles were used.

Lullabies were sung to babies. A very old informant squatted on the ground, "rocked" herself and her great grandchild, which she was holding on her knees, back and forth, singing a two-syllabled lullaby during interims when the information she had given was being relayed by the interpreter.

NAVEL CORD

After the navel cord dropped off, the mother dried it thoroughly, rolled it into a little ball, and then sewed it into a beaded buckskin bag. A girl's bag was usually round or diamond-shaped; occasionally it represented an alligator. A boy's was generally turtle-shaped; occasionally one was diamond-shaped.

The mother kept the child's bag attached to its clothing in the region of the shoulder blades until the child reached the age of reason.[7] The bag was never attached to the cradle. After the child showed signs of reasoning, it was given its own bag to keep and care for. A girl usually wore hers attached to her clothing. The proper place was at the shoulders, but some girls wore theirs attached to the belt. The daughter of a Southern informant in 1935 had hers—an elongated diamond-shaped one—attached to the back between the shoulders on a beaded buckskin dress. The boy sometimes wore his attached to his belt; more often he hung it on the wall of the tipi. Children were taught to treat their navel bags with respect. The bag was to be near its owner, at least until the owner reached maturity, for it was believed that its presence helped the child or youth "to be good and not to steal or do other disreputable things." If it was noticed that a child who was getting restless and mischievous was not wearing its navel bag, someone would say to it: "Are you looking for your navel? You'd better find it! And wear it too!" The cord was never burnt. Burning it would cause the child to be constantly digging around in ashes and embers.

NURSING AND WEANING

A child was not nursed by its mother until it was four days old. When it uttered its first cry, it was given a decoction made by boiling the bark of the wild rose minus its thorns. After that it was nursed by a woman who was already nursing her own two- or three-month old baby. A child was nursed whenever it cried and not at set intervals.

A medicine woman, or just as often the mother of the delivered woman, suckled the woman's breast for the four days following delivery when she was not nursing her child. Most women kept breasts warm with a covering of fur. For several days after delivery they refrained from drinking cold water since

[7] Grinnell observed that little deerskin charms containing the dried fragments of the umbilical cord were tied to the clothing of the children up to the age of six or seven years; they often had the shape of a lizard or turtle (1923, 2: 110).

it might cause pneumonia. Warm decoctions of two kinds were recommended by Southern women. One was made by boiling "a plant that grows in plowed fields; it has no leaves and is all stalks. It looks blue-green, except at the top where it has a yellow appearance. The other is a weed gathered in Wyoming in the spring. To help the milk-flow we made a tea from an herb that grows near here" (Anadarko, Oklahoma).[8]

To prevent hemorrhages the woman was not to eat sour foods or boiled meat for four days following delivery. She could eat barbecued meats, however. Some were known to have eaten soups with dried meats well pounded.

Children were nursed until the mother's milk flow ended, unless she became pregnant, in which case she removed the nursing child to the home of a relative. If this was not convenient, she applied the essence of a bitter root to her nipples, thus repelling the child. Children were usually nursed until they were four or five years old; some were known to have nursed seven, eight, and nine years. Today most children are nursed until they are two years old. "But my neighbor's boy over there is four years old now (1936). I often see him playing around, and when he is hungry, he runs to his mother and nurses." "I saw one Cheyenne woman nurse her nine-year-old child recently (1940)." A woman was seen nursing her two-year-old twins, one on each breast. One child stood on each side.

Women spaced children by nursing them as long as possible. In the days of polygynous marriages, nursing women usually let their co-wives live with the husband while they themselves lived apart in another tipi.

ATYPICAL CONDITIONS

No triplets were known to have been born to any Cheyenne. Twins were born occasionally. Twins were not desired, but once born they were accepted. Because they were unusual, not because any supernatural power was attached to them, they were treated better than their brothers and sisters. Usually they were given better clothing and the best portions of food. They had to be treated exactly alike, for if one was treated better than the other, jealousy might arise. The one less well treated might even become ill and die. It was for this reason that only one was nursed on each breast during a feeding.

No case of incest was known. "When we would hear of that as having happened in other tribes or among Whites, our people would say that any one doing that must be crazy; that such persons had no self-respect."

A child born out of wedlock was not considered disgraced. It was never killed. It was born in the home of its mother's parents and reared there unless the mother married its father, in which event it was taken to the home of its

[8] Informants were not willing to identify the plants any farther. Grinnell writes that the mother was given *motsiiyun*, the milk medicine (*Actaea arguta*), to induce a free flow of milk (1923, 1: 148).

parents. A woman conceiving out of wedlock lost her reputation in the community. Her family upbraided her for her conduct and urged her to marry the man. She was not, however, ostracized from her home if she failed to marry him. "Such a girl was thought of as having been dishonest with her parents. The girl was told to marry the man. If she did not know who the father of her child was—which might happen if she had been promiscuous or had conceived while being intoxicated—the people were able to point out the father after the child was a year or two old; it would in some way resemble its father. Then the man would be spoken of as the child's father and told to marry the girl." An unmarried father was in no way punished by the tribe, nor did his reputation suffer to the same extent as did the unmarried mother's.

NAMES

Generally a child was given a name on the day of birth, if not then, at least within a few days. It was customary for an old person, usually a paternal relative, to announce himself as the namer of a child before the child's birth. Sometimes the parents asked an old person to be the namer. If no namer was available, parents named the child.

A Northern informant was certain that only old men, never women, named children, both boys and girls. A Southern informant knew of both men and women who had named both boys and girls. She remarked, "Before my first child was born, I was hoping that it would be a boy. One day a medicine woman dropped in and said, 'I will name your child. If it is a boy, I'll name him Little Playing Buffalo Calf; if a girl, Little Playing Buffalo Woman.' The baby was a boy and she named him Little Playing Buffalo Calf. Boys were always named after something big or great, such as the buffalo, the thunder, or birds or animals of some kind. Girls might be given similar names, but all girls' and women's names ended in the word 'woman.' My daughter's name is Thunder Woman. One day, during the time that I was carrying her, a woman came to see me. She did not speak of naming the baby then, but later I was told that she had it in mind then. After the baby was born she came again, and said, 'I'll name that child so that it will be healthy and strong, and that it will live to be as old as I am. I will name it Thunder Woman. If you do not call it by this name, it will not be blessed.' "

Gifts for the baby were often given to the mother by any one wishing to do so, after the baby was named. "My aunt gave me a beaded cradle for my first baby." Following the naming a feast was given by the parents for the namer and others, always among them old people.

A nickname, generally retained all through life, characterized the person because of some atypical physical or mental trait. "One of my sons is called Crooked Foot. That is his nickname. A woman who visited us the day he was born noticed that his right foot was slightly crooked, and so she gave him that

name. He often says now that if he could locate that woman, he thought he would do something to her for giving him such a name." "My real name is Buffalo Woman, but I'm called White Woman. My aunt and uncle gave me that as a nickname because I was so fair-skinned."

Some Cheyenne retained for life the name given them in babyhood. Sometimes parents chose to have a pre-adolescent child's name replaced by a new name, one that was valued because it had been in the relationship for many years. Parents asked a grandparent of the child to bestow it. Cheyenne often changed their names in adulthood. "I have had two Indian names. After I was born, I was named Feather Robed Woman. That was the name of my father's departed sister. After I was older, they named me Crippled Nose Woman after a crippled aunt, a relative of my father who was crippled, and whom I was to care for. After she died, they again called me Feather Robed Woman." "A man may take a new name after killing an enemy in war, or when accidentally coming upon an enemy and killing him, while hunting. For example: When we were acting out Custer's last stand here on the Tongue River for the Paramount Picture Corporation recently (1936), my husband, Wolf Tooth, acted the part of the one that led the enemy into Custer's camp. He killed Custer. After we had acted out this part, we had a dance. We all painted our faces with charcoal and danced around Wolf Tooth for killing Custer. Then one man made a motion as though he were throwing something on the ground and said 'Here lies the name of Wolf Tooth!' Some one called out then 'Howling Wolf Tooth!' And now that's my husband's name. At the Office (reservation office of the United States Bureau of Indian Affairs) his name remains Wolf Tooth, however."

Names may have originated in dreams; informants disagreed on this point.[9] They did agree that names were coined upon encountering some unusual object, animal, or person, or because of an unusual experience. "A certain man called a child White Turtle because he had seen a white turtle while he was fishing." A name once owned by a person of note was retained in the relationship. If the person had been very renowned and had done much for the tribe, his relatives felt that any one in the tribe had a right to his name. Such a name was either given to others or taken by them. "I named one of my grandchildren White Bird after a cousin of mine who had been dead a long time. He was a good-natured fine man. Names were often chosen because people who had owned them had been good and of some account."[10]

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[9] The names of the Chippewa originated in dreams. For a complete account of the origin of a Chippewa name see my article, Ceremonia para dar nombre a un niño indio Chippewa, *America Indigena*, July 1944, Vol. IV, pp. 237–242.

[10] Grinnell writes that a Cheyenne child's first name was a pet name, that is, a name of endearment. A little boy was sometimes called *Moksois*, meaning Pot Belly; a little girl, *Moksiis*. The child was given a more formal name when it was five, six, or seven years old. A child usually had a nickname by which it was generally known (1923, 1: 107).

The Cheyenne had no surnames. Surnames were imposed on them by the Whites. Every Cheyenne was known to the tribe by his own individual given or assumed name. "My three brothers are called Antelope, Little Bear, and Stone. These are their Indian names. Antelope and Little Bear went off to boarding school. There they were given Christian names and their Indian names were used as surnames. And so it happened that my brothers' Indian names became the surnames of their families. Antelope has a little girl that everyone calls Sally Antelope, and Little Bear's girl is called Mary Little Bear."

Surnames of Cheyenne buried in St. Labre's Mission cemetery, Montana, were Bear Comes Out, Tall White Man, Kingfisher, Yellow Horse, Standing Elk, Magpie, Big Head, Yellow Fox, Sponge, Swallow, Two Feathers, Yellow Bird, Holy Bird, Yellow Hair, Spotted Hawk, Yellow Eye, Red Neck, Wolf Chum.

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BIBLIOGRAPHY

DE SMET, P. J.
1881 *Western Missions and Missionaries*. New York.

GRINNELL, GEORGE BIRD
1923 *The Cheyenne Indians*. 2 vols. Yale University Press.

HILGER, SISTER M. INEZ
1936 *Chippewa Prenatal Food and Conduct Taboos*. Primitive Man, July. Washington.
1944 *Ceremonia para dar nombre a un niño indio Chippewa*. America Indigena, 4, 237–242. Mexico City, 1944.
N.D. *Chippewa Child Life*. Bureau of American Ethnology, Smithsonian Institution, Washington. (Forthcoming publication.)
N.D. *Arapaho Child Life*. Bureau of American Ethnology, Smithsonian Institution. Washington. (Forthcoming publication.)

MOONEY, JAMES
1905–07 *The Cheyenne Indians*. Memoirs of the American Anthropological Association, 1.

ROBERT BENNETT BEAN, 1874-1944

By R. J. TERRY

PROFESSOR Robert Bennett Bean, distinguished for his work in anthropology and human anatomy, died in Staunton, Virginia, August 27, 1944. His active career in science began with his graduation in medicine in 1904 and continued without interruption to the time of his retirement, caused by ill health, from the professorship of anatomy in the University of Virginia, 1942.

R. Bennett Bean was born March 24, 1874 at Gala, a town at the sources of the Potomac River, in Botetourt County, Virginia. He was one of eight children from the marriage of William Bennett and Arrianna Williamson Carper Bean. Through his mother he was descended from William Randolph (1650-1711), a founder of William and Mary College, and an ancestor of Thomas Jefferson, John Marshall and Robert E. Lee. Bennett's childhood and youth were spent in the country where the foundation of his education was laid in contacts with nature and in home tutelage, with much inculcating of religion and reading of the Bible. At the age of twenty-six (1900) Bean received the degree of B. S. on the completion of collegiate studies in the Virginia Polytechnic Institute and entered Johns Hopkins Medical School.

Available records of Dr. Bean's life fail to explain his choice of the study of medicine. Having entered upon the pursuit of this branch of biological science, his inquiring mind was fascinated by the challenge of problems presented by morphology. In this interest he was encouraged by Professor Mall, for after receiving the M.D. degree in 1904 he was appointed to the post of assistant in anatomy at Hopkins. At this early moment anthropological tendencies are revealed in Dr. Bean's researches, for they were concerned with observations on the subclavian artery and on the brain in series of Negroes and Whites, subjects that continued to hold his interest in later years.

From 1905 to 1907 Dr. Bean was instructor in anatomy at the University of Michigan, in that period pursuing research on the brain of the Negro and entering the field of anthropometry with measurements taken on Ann Arbor students. The exacting techniques of measuring as applied to anthropology were gained in Paris under Manouvrier during the summer of 1906.

The years of 1907 to 1910 were ones of marked activity in Dr. Bean's research and teaching. He was called to Manila as assistant professor and director of the anatomical laboratory in the Philippine Medical School. Seventeen titles of published works dealing with studies of Philippine types are to be noted in the list of Dr. Bean's writings while in Manila. His book "Racial Anatomy of the Philippine Islanders" appeared at this time. These studies constitute important contributions to our knowledge of the somatology of these peoples.

In the year 1910 Dr. Bean became associate professor, and later professor of anatomy, at Tulane, remaining in this laboratory until 1916 when he was

called to the University of Virginia. While he continued publishing results of Philippine investigations it is to be noted that at this time several of his papers are of a deductive nature, constructed on foundations of his own observations and on those of others. It was while in New Orleans that his interest was aroused and investigations were begun concerning the question of correlation between somatologic types and disease. And it was during this period too that Professor Bean initiated his series of studies on the weights of organs relative to type, race, sex and age, studies carried on in the years following at Virginia.

Professor Bean returned to the State where he was born and to the headship of the department of anatomy of the University of Virginia in 1916. There he had a large share in effecting the progressive changes that culminated in the present fine medical institution in Charlottesville. Outside of his professional activities he entered with spirit into the programmes of civic organizations and served steadfastly in religious work as member and vestryman of the Episcopal Church.

In research during the twenty-six years at Virginia ending with Professor Bean's retirement, some of his old problems yielded new papers and new fields of interest were entered upon. His important contributions to the subject of the postnatal growth of the heart and other organs were made in this period. Several papers appeared in 1921 concerning medical education and the teaching of anatomy. His later ideas regarding human types with reference to race and in relation to disease are set forth in a number of papers. In 1932 appeared his book "Races of Man." Anthropologic study of old Virginians, begun in 1930, brought out a series of valuable contributions. The completion and publication of a third book, "The Peopling of Virginia," was achieved in 1938, four years before Professor Bean's retirement.

As a teacher Bean enjoyed the work of the classroom; he was enthusiastic in his efforts with students to draw out their understanding, was stimulating by his wide knowledge of anatomy and anthropology, by his ingenious methods for clarifying the obscure, by his example in research. This writer in his many relations with Dr. Bean found his manner informal and cordial; his students noted a degree of reserve and marked dignity manifested in dealing with the subject of his lecture and toward his auditors.

Among the many scientific societies of which Professor Bean was a member may be mentioned: the American Association of Anatomists, American Anthropological Association (councilor, 1919), American Association of Physical Anthropologists, American Geographical Society, Sigma Xi, Fellow of the American Association for the Advancement of Science (chairman, section H, 1926), Corresponding Member of the Societa Romana Antropologica, Academy of Science of Virginia, New Orleans Academy of Science (president, 1915).

For the list of Professor Bean's publications following this article I am in-

debted to Professor Harvey E. Jordan, Dean of the School of Medicine, the University of Virginia.

Dr. Bean married, May 22, 1907, Adelaide Leiper Martin, who survives him. Their four children, all living, are: Mary Archer, born in Baguio, P. I., married to James van Deusen Eppes; William Bennett, born in Manila, P. I., Major, M.C., AUS; Helen Homes, born in New Orleans, La., married to Natt Morrill Emory; George Martin, born in University, Va.

Bean was a leader in his field of research and in the several activities outside of his profession into which he entered. His passing deprives anthropological science of an original mind and an indefatigable worker and contributor; the loss of his fine example in supporting worthy endeavors in the communities in which he lived is keenly felt, and his death deeply deplored by his many friends.*

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BIBLIOGRAPHY

1904 *Observations on a study of the subclavian artery in man.* Johns Hopkins Hosp. Bull., 15: 203-205.

1905 *A composite study of the subclavian artery in man.* Am. Jour. Anat., 4: 303–328.

On a racial peculiarity in the brain of the Negro. Assoc. Am. Anat. Proc., 1904–05, p. iv.

1906 *Some racial peculiarities of the Negro brain.* Am. Jour. Anat., 5: 353–432.

The Negro brain. Century Magazine, 72: 778–784.

Training of the Negro. Century Magazine, 72: 947–953.

1907 *A preliminary report on the measurements of about 1,000 students at Ann Arbor, Michigan.* Anat. Rec., 1: 67–68.

A racial peculiarity in the temporal lobe of the Negro brain. Anat. Rec., 1: 57.

1908 *Lectures on heredity.* Teacher's Assembly Herald, Baguio, Benguet, Philippine Islands.

A theory of heredity to explain the types of the white race in North America. Philip. Jour. Sci., Sect. A., 3: 215–233.

Benguet Igorots: A somatologic study of the life folk of Benguet and Lapanto-Bontoc. Philip. Jour. Sci., Sect. A., 3: 413–467.

1909 *Methods of studying the structure of the central nervous system.* Philip. Jour. Sci., Sect. B., 4: 9–19.

Filipino ears: A classification of ear types. Philip. Jour. Sci., Sect. A., 4: 27–53.

Filipino types: Manila students: an attempt to classify the littoral population of Luzon and adjacent islands. Philip. Jour. Sci., 4: 263–296.

Filipino types found in Malecon morgue. Philip. Jour. Sci., 4: 297–337.

Filipino types: Racial anatomy in Taytay. A. *The men* (*With Planta, F. S.*) Philip. Jour. Sci., 4: 359–446.

A cephalograph: The description of an instrument for reproducing the outlines of the head and face. Philip. Jour. Sci., Sect. A., 4: 447–449.

A scheme to represent type heredity in man. Science, 29: 942–944.

1910 *Racial anatomy of the Philippine Islanders.* pp. 236, Lippincott.

Ear as a morphologic factor in racial anatomy. Verh. d. VIII. Intern. Zool. Kong. zu Graz, pp. 921–925.

Types of Negritos in the Philippine Islands. Am. Anthrop., 12: 220–236.

* A full obituary may also be found in *Science*, 101, No. 2623, pp. 346–348. (Editor)

Philippine types. Am. Anthrop, 12: 377–389.
Filipino types; racial anatomy in Taytay. B. *The Women (With Planta, F. S.)* Philip. Jour. Sci., 5: 1–25.
Paleolithic man in the Philippines; Homo Philippinensis. Philip. Jour. Sci., 5: 27–31.
Filipino ears II. Ears from the Malecon morgue. Philip. Jour. Sci., 5: 191–195.

1911 *Heredity of hair from among Filipinos*. Am. Natur., 45: 524–536.
The men of Cainta (With Planta, F. S.) Philip. Jour. Sci., Sect. D., 5: 7–15.
Filipino ears III. Negrito. Philip. Jour. Sci., 6: 107–125.

1912 *Study of physiognomy: The evolution of the human face*. Am. Antiq. & Oriental Jour., 34: 265–271; 35: 231–236.
Some useful morphologic factors in racial anatomy. Anat. Rec., 6: 173–179.
Morbidity and morphology; a composite study of the incidence of disease and physical form in New Orleans, Louisiana. Johns Hopkins Hosp. Bull., 23: 363–370.
Some factors in the differentiation of human types. Science, 35: 674–675.

1913 *Notes on the hairy men of the Philippine Islands and elsewhere*. Am. Anthrop., 15: 415–424.
Three forms of the human nose. Anat. Rec., 7: 43–46.
The nose of the Jew and the quadratus labii superioris muscle. Anat. Rec., 7: 47–49.
The cephalic nerves: suggestions. Anat. Rec., 7: 221–235.
Filipino ears IV. Ilongot and Mangyan. Philip. Jour. Sci., 8: 357–358.
Types among the island tribes of Luzon and Mindanao. Philip. Jour. Sci., Sect. D., 8: 455–462.

1914 *The stature and the eruption of the permanent teeth of American, German and Filipino children. Deductions from the measurements and examination of 1,445 public school children in Ann Arbor, Michigan and 776 in Manila, P. I*. Am. Jour. Anat., 17: 113–160.
The eruption and decay of the permanent teeth; preliminary report. Anat. Rec., 8: 299–302.
A racial peculiarity in the pole of the temporal lobe of the Negro brain. Anat. Rec., 8: 479–491.
Reform in education. Pedagog. Sem., 21: 284–286.

1915 *The growth of the head and face in American (white), German-American and Filipino Children*. Am. Anthrop., 17: 525–528; also Anat. Rec., 9: 50-52.
Some ears and types of men. Am. Anthrop., 17: 529–533.
Some characteristics of the external ear of American Whites, American Indians, American Negroes, Alaskan Esquimos and Filipinos. Am. Jour. Anat., 18: 201–225.
Some ears and types of men. Anat. Rec., 9: 52–55.

1916 *Notes on the alimentary canal of the hyper-ontomorph and the meso-ontomorph*. Anat. Rec., 10: 181.
Disease and death rate in human types. New Orleans Med. and Surg. Jour., 69: 175–190.
The permanent teeth, with special reference to American children. Science, 53: 866.

1917 *The weights of the organs in relation to type, race, sex, stature and age*. Anat. Rec., 11: 326–328.

1919 *Some racial characteristics of the spleen weight in man (With* Wilmer Baker) Am. Jour. Phys. Anthrop., 2: 1-9.
Some racial characteristics of the liver weight in man (With Wilmer Baker) Am. Jour. Phys. Anthrop., 2: 167-173.
Some racial characteristics of the weight of the heart and kidneys (With Wilmer Baker) Am. Jour. Phys. Anthrop., 2: 265–274.
The weight of the leg in living men. Am. Jour. Phys. Anthrop., 2: 275-282.

1920 *The adult sitting height*. Anat. Rec., 18: 222.
The sitting height in children. Anat. Rec., 18: 222–223.
Notes on the postnatal growth of the heart, kidneys, liver and spleen in man. Carnegie Inst. Washington. Contrib. to Embryol., 9: 263-284.

1921 *Remarks on teaching anatomy*. Anat. Rec., 21: 391–399.
Anthropology in the medical curriculum. Science, 54: 371–373.
A vital problem of medical education. Virgin. Med. Month., 47: 504–506.

1922 *The sitting height.* Am. Jour. Phys. Anthrop., 5: 349–390.

1923 *The two European types.* Am. Jour. Anat., 31: 359–371.

A note on the head form of 435 American soldiers with special reference to flattening in the occipital region (*With* C. C. Speidel) Anat. Rec., 25: 301–311.

Notes on the body form of man. Eugenics in Race and State, 2: 7–24.

Die Morphologie und die Erkrankungen des Menschen. Zeitschr. f. d. gesamte Anat.: Zeitschr. f. Konstitut., 9: 439–459.

1924 *The three anatomic types of Africa.* Am. Jour. Anat., 33: 105–118.

The pulse of growth in man; a preliminary report. Anat. Rec., 28: 45–61.

Notes on the index of the foot among young white men (*With* C. T. Burton) Anat. Rec., 28: 165–171.

1925 *Some anatomical characters of the mongoloid, a hypomorph white type.* Am. Assoc. for the Study of Feeblemindedness. Proc., 30: 293–312.

Types of man in yellow-brown race. Am. Jour. Anat., 35: 63–80.

Materials and methods for the study of the heart, liver, spleen and kidneys with their average weight and the relation of weight to sex and race. Anat. Rec., 29: 347–348.

A four dimensional table for expressing the arrays of four factors. Anat. Rec., 29: 348.

The weights of the heart, liver, spleen and kidneys in relation to age, stature and condition of nourishment. Anat. Rec., 29: 348.

The relation of the heart, liver, spleen and kidney weight to certain diseases (*With* E. O. Barr) Anat. Rec., 29: 349.

Preliminary notes on a study of mongoloid dwarfs (*With* F. H. Wilke) Anat. Rec., 29: 349.

A preliminary survey of more than eleven hundred abnormal children. Anat. Rec., 29: 349–350.

Some anatomical characters of the mongolian idiot. Virgin. Med. Month., 51: 642–644.

1926 *Types of the three great races of man.* Am. Jour. Anat., 37: 237–271.

Composite study of weight of vital organs in man. Am. Jour. Phys. Anthrop., 9: 293–319.

Human types. Quart. Rev. Biol., 1: 360–392.

Notes on the weight of some vital organs; their development; variability and relation to disease; summary. Virgin. Med. Month., 52: 692–697.

1927 *Symposium on some biological aspects of medical problems: Human types in relation to medicine.* Am. Natur., 61: 161–172.

Humanity is divided into four main types. New York Times, March 7.

1928 *Stature throughout the world.* Science, 67: 1–5.

1929 *A unique model of the entire peritoneum.* Anat. Rec., 41: 387–393.

1930 *Some measured characters of old Virginians.* Anat. Rec., 45: 255–256.

Old Virginians. Anat. Rec., 45: 289.

The growth of old Virginia children: A comparative study; tentative conclusions. South. Med. and Surg., 92: 871–872.

1931 *Stature in old Virginians.* Am. Jour. Phys. Anthrop., 15: 355–419.

1932 *"The Anthropometry of the American Negro"* By M. J. Herskovits, reviewed in Am. Jour. Phys. Anthrop., 17: 257–259.

Anatomy of pelvis and lower extremity. (*In* George Blumer's Practitioner's Twentieth Century Medicine and Surgery, v. I., Chapter 7, pp. 601–675).

The races of man. 134 pp. N. Y. University Society.

1933 *Sitting height and leg length in old Virginians.* Am. Jour. Phys. Anthrop. 17: 445–479.

1934 *The cephalic index, head length and breadth in old Virginians.* Am. Jour. Phys. Anthrop., 19: 247–288.

1935 *Hair and eye color in old Virginians.* Am. Jour. Phys. Anthrop., 20: 171–204.

1938 *Peopling of Virginia.* 302 pp. Chapman and Grimes.

JESSE DADE FIGGINS, 1867–1944

By H. M. WORMINGTON

JESSE D. FIGGINS died at Lexington, Kentucky, on June 10, 1944, at the age of 77. Although Mr. Figgins was best known as a naturalist and museum executive his death was a loss to archeology, for his contributions to the subject had changed the whole perspective of studies of New World prehistory. It was through his recognition of the significance of the association of man-made objects and the bones of extinct bison at the Folsom type station, and his unremitting efforts to have this site properly authenticated that the chronological horizon for man in North America was pushed back many centuries. His sponsoring of the excavation of one of the first sites where projectile points were found in unmistakable association with mammoth was of the greatest importance, and his published reports have contributed much to the literature pertaining to the antiquity of man on this continent.

J. D. Figgins was born on August 17, 1867, in Frederick County, Maryland. His first scientific interest was ornithology and for some time he was employed by the United States National Museum. Later he joined the staff of the American Museum of Natural History and, as head of the Department of Preparation and Exhibition, began the construction of large habitat groups with painted backgrounds for which he was famous.

In 1910 Mr. Figgins became director of the Colorado Museum of Natural History and served in that capacity until 1936 when he resigned in order to build another museum for the Bernheim Foundation in Kentucky. When wartime conditions made it impossible to continue this project he became affiliated with the University of Kentucky. Under Mr. Figgins' direction many fine exhibits were created at the Colorado Museum. His skill as a painter enabled him to create backgrounds which made mounted animals appear to be alive and in their natural surroundings. Although most of his time was necessarily devoted to administrative duties and painting Figgins did much research work in a variety of fields including ornithology, mammalogy and paleontology.

Mr. Figgins first became interested in archeology in 1926 when field workers of the Colorado Museum of Natural History reported to him the finding of two pieces of chipped stone in a deposit in which they were excavating the bones of an extinct species of bison. Later a third piece, imbedded in a block of matrix containing a rib bone, was sent to his laboratory. Mr. Figgins realized at once the importance of this find and during the following year he devoted much time to trying to interest archeologists in the discovery. In spite of the evidence presented the belief persisted that man had not been a contemporary of the long-extinct bison. Mr. Figgins, however, was firmly convinced of the authenticity of the association and continued the excavations

during the following field season. After four broken points had been uncovered in the course of excavating the fossil bison bones a fifth was found still imbedded in the matrix. Mr. Figgins immediately sent telegrams to leading scientific institutions with the request that they send qualified witnesses to view the new find *in situ*. Dr. Barnum Brown, Dr. Frank H. H. Roberts, Jr., and Dr. A. V. Kidder were among those who came in reply to Mr. Figgins' request. The evidence was clear, and with the acceptance of the authenticity of the association by well-known archeologists, belief in the antiquity of man on this continent grew rapidly.

An account of the discovery, published in 1927 in "Natural History" by Figgins, did much to stimulate interest in the problem of early man. In 1928 a joint expedition from the Colorado Museum of Natural History and the American Museum of Natural History, headed by Dr. Barnum Brown, furnished still more evidence.

In 1931 a fluted projectile point was found in association with an articulated mammoth skeleton near Angus, Nebraska by A. M. Brooking. This find was studied and reported upon by Figgins. Again in 1933 Mr. Figgins was responsible for focusing the attention of archeologists on an association of fluted points and mammoth remains. While excavating a deposit of mammoth bones near the town of Dent, Colorado, in 1932, Father Conrad Bilgery, S. J., found a fluted point associated with the articulated bones of mammoth. Father Bilgery offered the Colorado Museum of Natural History the privilege of continuing the excavation of the site and the following year a second point was uncovered under Mr. Figgins' direction. Thanks to him the Dent Site is one of the most fully witnessed and documented. Still and motion pictures were taken and the point was removed imbedded in the matrix.

The study of Yuma artifacts was also greatly advanced by Figgins. Not only did he study, photograph, and report on many Yuma points but he also provided museum sponsorship and funds for others engaged in studying the problem.

J. D. Figgins' archeological work was marked by his realization of the need for the careful recording of all data and the importance of having competent witnesses present when important finds were made. Two characteristics which were evident in all his work were a tremendous enthusiasm and the ever present curiosity of the scientist. It is thanks to these traits that we know as much of the Folsom and other ancient complexes as we do today. Without Mr. Figgins, knowledge of the antiquity of man in America might not have come for many years.

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BIBLIOGRAPHY*

1927 *The Antiquity of Man in America*. Natural History, vol. XXVII, No. 3, pp. 229–239.

1931 *An Additional Discovery of the Association of a "Folsom" Artifact and Fossil Mammal Remains*. Proceedings, Colorado Museum of Natural History, Vol. X, No. 2.

1933 *A Further Contribution to the Antiquity of Man in America*. Proceedings, Colorado Museum of Natural History, Vol. XII, No. 2.

1934 *Folsom and Yuma Artifacts*. Proceedings, Colorado Museum of Natural History, Vol. XIII, No. 2.

1935 *New World Man*. Proceedings, Colorado Museum of Natural History, Vol. XIV, No. 1.

1935 *Folsom and Yuma Artifacts*, Part II. Proceedings, Colorado Museum of Natural History, Vol. XIV, No. 2.

* Only those publications pertaining to anthropology are here included.

KROEBER'S "CONFIGURATIONS OF CULTURE GROWTH[1]"

By LESLIE A. WHITE

THE publication of "Configurations of Culture Growth" is an event of more than ordinary interest. It is not just "another book on anthropology." It is a huge work, the result of much thought and prodigious labor, the mature fruit of an exceptionally able and energetic mind, one that has been occupied with anthropological theories and problems for over forty years. Since this work embodies and reflects many trends and views in American anthropology since Boas went to Columbia in 1896 it would be well, in order to appreciate its full significance, to view it against this historical background.

Franz Boas was at variance with the outlook and procedure of science in a number of respects, only one of which, however, interests us here, namely, his hostility to theory, his incapacity for creative synthesis, his aversion for generalizations and systems. As Lowie puts it: "He abhorred facile generalizations and all-embracing systems."[2] Boas apparently took pride in this attitude and a number of his disciples have praised him for it. They call it "scientific rigor" "merciless logic," "acidly critical," etc. Without theory, creative synthesis and generalizations there can, of course, be no science. And as for "all-embracing systems," the more comprehensive the better. As a matter of fact, it is the possibility of reaching "a unifying theoretical basis for all . . . single sciences, consisting of a minimum of concepts and fundamental relationships, from which all the concepts and relationships of the single disciplines might be derived" that has been, according to Einstein,[3] "the chief source of the passionate devotion which has always animated the researcher."

Instead of going from the particular to the universal, which is the course followed everywhere in science, Boas went from the particular to further particulars. "The detailed study of particulars seemed to him more rewarding than the building of systems" (Benedict).[4] Not only did he fail to see the forest for the trees, he could scarcely see the tree for the branches, or the branches for the twigs. And no two twigs were alike. Diversity is what impressed him. The ultimate was the concrete event which was, of course, unique.

The picture of culture that Boas produced, working from this point of view, was precisely what might have been expected. There was no order, no rhyme or reason, to the great mass of cultural phenomena that make up the history and life of mankind. As R. H. Lowie has so neatly expressed it, civilization appeared to be a "planless hodge-podge, a chaotic jumble."[5]

If Kroeber once accepted this view as a student of Boas, he has also endeavored to break away from it. In various writings scattered over a quarter

[1] University of California Press, pp. x+882, $7.50, Berkeley, 1944.

[2] Lowie, 1943, p. 184. (See Bibliography at end of article.)

[3] Einstein, 1940, p. 488. [4] Benedict, 1943, p. 33. [5] Lowie, 1920, p. 441.

of a century he has tried to bring order out of chaos, to render culture intelligible, in short, to produce an ethnology that makes sense. In reviewing Lowie's "Primitive Society," which I along with many members of the Boas group regard as one of the finest expressions of the Boasian point of view, Kroeber wrote as follows:[6]

> . . . what can be said of the value of this method [i.e., the one exemplified in *Primitive Society*]? This admission seems inevitable: that though the method is sound, and the only one that the ethnologist has found justifiable, yet to the worker in remote fields of science, and to the man of general intellectual interests, *its products must appear rather sterile*. There is little output that can be applied in other sciences. There is scarcely even anything that psychology, which underlies anthropology, can take hold of and utilize. There are in short no causal explanations. . . . In essence, then, modern ethnology says that so and so happens, and may tell why it happened thus in that particular case. It does not tell, and does not try to tell, why things happen in society as such. . . . As long as we continue offering the world *only reconstructions of specific detail, and consistently show a negativistic attitude toward broader conclusions, the world will find very little of profit in ethnology. People do want to know why.* (Emphasis ours.)[7]

Instead of seeing civilization as a "planless hodge-podge, a chaotic jumble," Kroeber discovers "a majestic order pervading civilization."[8] He writes an essay "On the Principle of Order in Civilization as Exemplified by Changes of Fashion."[9] He has attempted to work out a logic of the sciences in which the position of anthropology with reference to the other sciences is indicated in such articles as "The Superorganic,"[10] "The Possibility of a Social Psychology,"[11] and "So-Called Social Science."[12] "Configurations of Culture Growth" is simply a continuation, and the most impressive example, of Kroeber's attempt to find his way out of the Boasian wilderness, where there was no rhyme or reason, into a world that is intelligible, one that makes sense.

In the first place, Kroeber sees culture, not as random and unrelated phenomena, but as events organized into patterns. A pattern is a system, a manifestation of order. Secondly, culture patterns grow, and these growths themselves follow patterns. Kroeber describes their life cycle in such terms as " 'growth,' 'realization,' 'exhaustion', and 'death' " (p. 91),[13] or "growth, saturation and exhaustion" (p.13). A culture pattern is "a system [which] bears in itself certain specific potentialities, and also specific limitations, which

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[6] Kroeber, 1920, p. 380.

[7] Twenty-three years later, Clyde Kluckhohn wrote of Boas as follows: "Tell an acute psychologist, economist, sociologist to read Boas' article Anthropology in the Encyclopedia of the Social Sciences, and—it is an induction from my experience—he comes away disappointed and feels empty-handed" (Kluckhohn, 1943, p. 210).

[8] Kroeber, 1917, p. 201. [9] Kroeber, 1919.

[10] Kroeber, 1917. [11] Kroeber, 1918a. [12] Kroeber, 1936.

[13] Unless otherwise indicated, page numbers in parentheses refer to *Configurations of Culture Growth*.

enable it, even compel it, barring catastrophe from outside, to realize or fulfill itself to the terminus of these potentialities, but not to go beyond them. When the given set of potentialities is exhausted, the growth ends . . . there is no further growth until a thoroughly new set of impulses has arisen under new circumstances. . . ." (p. 90).

Thus we have configurations of growth of culture patterns. To study these configurations is the task Kroeber has set himself in this work. He therefore undertakes a survey of the development of various aspects of culture, such as philosophy, science, philology, sculpture, painting, drama, literature and music (each subject comprising a chapter, Chs. 2–9, inclusive), for all the countries or regions of the world that have produced advanced cultures and about which enough is known to make such a treatment possible. To these he adds a chapter on "The Growth of Nations" (Ch. 10).

To give the reader a better idea of Kroeber's method and procedure, let us examine the chapter on Science (105 pp.) in some detail. He begins with a few pages of "general" discussion. Then follow historical analyses of the growth of science in Greece, Rome, Arabia and Islam, Italy, Switzerland, France, the Low Countries, Great Britain, Germany, Scandinavia, Russia and the United States. Then after a brief discussion of "the general European course" of science, which he summarizes also in a chart, Kroeber turns to the history of science in Egypt and Mesopotamia, India, China and Japan. He again tries to pull his data together and finally to draw conclusions, the "most important" of which is that that portion of culture called "science" behaves much like other portions. Chapters on music, painting, philosophy, etc., proceed on the same plan.

On the basis of these surveys, Kroeber tries to answer, or to throw light upon the following questions: 1. How do the various configurations found in different regions and different historical periods compare with each other? Are the configurations of growth in philosophy in Greece, India and China, for example, similar, or do they differ? 2. Are the configurations of growth in one field, music, let us say, like those in another field, such as mathematics or sculpture? 3. To what extent do "the several patterns of one culture . . . [tend] to form, to culminate, and to dissolve or atrophy simultaneously"? (p. 822)

With regard to the first question, the results are largely negative. Kroeber finds all kinds of curves, "long and short, peaklike and plateaulike, fore-skew, aft-skew and symmetrical configurations" (p.776). It would have been interesting if he had tried to apply the S-shaped growth curve that seems to hold for a wide variety of biological phenomena, such as

> The population of the United States or France, decade by decade; the weight or height of a man, year by year; the number of yeast cells in a suspension or the size of bacterial colonies on an agar plate, hour by hour; and the length of a single bacillus, minute by minute—all of . . . [which] increase along the same shaped curve.[14]

[14] Gerard, 1940, p. 314.

This curve has the shape of an elongated and slanting S. At the beginning the rate of growth is slow, then it accelerates rather rapidly, and finally tapers off to a rather slow rate of increase again. As already stated, Kroeber finds all kinds of curves for growths in philosophy, music, sculpture, etc., but it seems likely that this S-shaped growth curve might be applicable to such phenomena as the growth of culture as a whole under such an impetus as the development of agriculture or the steam engine. Would we not find in each of these cases a slow growth at first, then a rapid acceleration, and eventually a tapering off?

Turning to the second question, Kroeber finds both similarities and differences among the configurations of growth in philosophy, music, drama, etc. And, as for the third point, he finds that "the several culture patterns of one culture" do tend to grow, mature, and decline more or less together, but here, too, there is considerable variation.

What then emerges from a comparison and analysis of these various historical sketches of the development of art, science, philosophy, etc.? What new light is thrown upon the configurations of culture growth, what new understanding is won? The answer is: very little. This is Kroeber's conclusion as well as mine: "I see no evidence," he says, "of any true law in the phenomena dealt with; nothing cyclical, regularly repetitive, or necessary. There is nothing to show either that every culture must develop patterns within which a florescence of quality is possible, or that, having once so flowered, it must wither without chance of revival (p. 761). . . . As for findings that are universal, or such as might express a general sociology of human history, this investigation has attained only to approximations. . . . " (p. 846). At the beginning of his final chapter, "Review and Conclusions," Kroeber says: "I confess to some lowering of expectations in the course of my work" (p. 762). Kroeber does not, apparently, regard his venture as a success, and the present writer is inclined to share this view. As far as I can see, we have but little clearer vision, but little deeper understanding, now than we had before. It may be worth while to try to discover the reason for this relative lack of success.

At the beginning of this review, we pointed out how Kroeber has tried for years to escape from the limitations of Boas' theoretical outlook. Whether he has done this consciously or not we are not in a position to say, but the fact is that he has deviated from the Boasian line on a number of occasions, and for his defections he has been taken to task or criticized by Haeberlin,[15] Sapir,[16] Goldenweiser,[17] and Benedict,[18] as well as by Boas[19] himself. He has tried to bring order out of a chaos that Boas only exaggerated. Since he has courageously taken and followed this course, one might perhaps expect more from "Configurations of Culture Growth" than is actually achieved. But the defects and shortcomings of Boas as a scientist were many, and to be rid of some

[15] Haeberlin, 1915. [16] Sapir, 1917. [17] Goldenweiser, 1917.
[18] Benedict, 1934, p. 231. [19] Boas, 1928, p. 235; and Boas, 1936.

is not to be rid of all. In our judgment, one of the chief reasons why "Configurations" has failed to achieve greater success is that it suffers from two theoretical errors, both traceable to Franz Boas.

The first is the confusion of the process that is history, a temporal sequence of unique events, with the process that is evolution, a temporal sequence of forms. Boas and his students recognize only two kinds of cultural processes, which they call "history" and "science." To them, "science" deals only with non-temporal processes. They confuse the two temporal processes, history and evolution, under one heading: "history." An historical account of events, however, is not at all the same as an evolutionist account. The *history* of horses, writing or money is quite a different thing from an account of their respective *evolutions*. The one deals with phenomena as unique events, with reference to specific time and place; the other deals with classes of phenomena without regard to *specific* time and place. The one particularizes, the other generalizes. By placing these two quite different types of process under the same heading, "history," the evolutionist process was made to disappear—which, of course, is what Boas was forever trying to accomplish. It also made it impossible for students of culture to see clearly and to think straight.[20]

Proceeding from this confused theoretical position, "Configurations of Culture Growth" tried to do the impossible: to generalize history. Kroeber begins with history, with concrete, unique events having specific time and place coordinates. He works with history, but out of history, by some sort of alchemy, he tries to get something else: evolution, or "growth," to use his own term. His attempt at transmutation fails, naturally; what he ends up with is, of course, *histories*. Although he says, "nor is there reason to believe that historians are wrong in having consistently refused to believe, or at least in doubting, the presence of strict universals in the record of human and social events" (p. 761) he nevertheless looks for them himself. Instead of finding universals or laws, however, he finds that "the growth-configuration approach results rather in a *multiplicity of specific historic findings*" (p. 846; emphasis ours). This is, of course, precisely what is to be expected. If one works along the lines of history, he will end up with histories—"a multiplicity of histories"—rather than with a universal. If Professor Kroeber had tried to compare the evolution of writing *as a process* (which he sketched in his "Anthropology"[21]) with the evolution of money, metallurgy, mathematics, etc., as so many examples of a kind of process, he *might* have been able to discover a universal, such as the S-shape growth-curve previously mentioned. As it is, "Configurations of Culture Growth" is another failure in the apparently perennial attempt to discover "laws of history."

[20] I have discussed the distinction between the historical and evolutionist processes at length in "History, Evolutionism and Functionalism: Three Types of Interpretation of Culture," 1945. See, also, "Science is *Sciencing*," 1938. [21] Kroeber, 1923, Ch. 11.

Although the theoretical weakness inherent in "Configurations" is to be traced to Boas, we believe that it would be unfair to shift the blame entirely to his shoulders. Kroeber seems to love facts, concrete, specific, unique facts, for their own sake, much as one might love the feel of velvet or the hard, chaste beauty of porcelain. At least he gives me this impression, and students in my courses who have tried to swallow and digest the shoals of facts in his "Anthropology" (especially chapters 14 and 15)[22] were apparently impressed the same way. In "Configurations," Kroeber tries to soar into the realm of universals, but he is unable to leave the solid earth of particular facts.

Another theoretical weakness of the work under review—or perhaps it would be more accurate to call it another aspect of the error previously discussed, since they are intimately related if not inseparable—is the notion that one can achieve universals, or laws, by the inductive method. Boas and his students have virtually made a fetish of induction, with a corresponding repudiation and ridicule of deduction. To Boas, deduction meant "unbridled imagination" and "wild conjecture"; deduction was a characteristic of "social evolutionists and other armchair philosophers." Induction on the other hand meant patient, laborious field work and research, the piling of fact upon fact, until something came of it. Generalizations arrived at deductively were easy, Boas argued, but scientifically worthless. Sound generalizations can be won only through induction. Thus Boas has maintained that "we recognize the fact that before we seek for what is common to all culture, we must analyze *each culture* by careful and exact methods. . . .[23] Unless we know how the culture of *each group* of man came to be what it is, we cannot expect to reach *any conclusions* in regard to the conditions controlling the general history of culture."[24] (Emphasis ours.)

This is a *reductio ad absurdum*. In the first place it is impossible to analyze the culture of each group of man by any kind of methods, exact or otherwise. All "cultures" except those of the present are, of course, extinct and can be observed, if at all, only through partial and fragmentary archeological remains. And of the living cultures, modern ethnographers have not been able to study *all* before they were profoundly modified through acculturation. Nowhere has science been able to examine each and every member of a class—every fish, every flower, meteorite, ray of light, atom of copper, etc. In the second place it is not necessary to do this in order to come to sound and valid generalizations, as every science will testify. And, finally, the basic principles, the great generalizations of science, are not arrived at inductively, as we shall see later.

Kroeber, too, worships at the shrine of induction. He praises Wissler, in reviewing "The American Indian,"[25] for his fidelity to the inductive method.

[22] *Id.* [23] Boas, 1900, p. 4.

[24] Boas, 1932, p. 609. Boas' hostility to deduction and his predilection for induction can be traced through his writings since 1887 at least. [25] Kroeber, 1918b, p. 208.

He undertakes to carry out his study of configurations of culture growth along inductive lines (p.vii). Consequently he amasses facts, mountains of them. His chapter on literature alone contains some 60,000 words, more than some entire books, such as Ruth Benedict's "Race: Science and Politics" or Redfield's "Tepoztlan." But what comes of all this massing of data? Masses of facts, "multiplicity of histories." They merely bear out the suppositions that Kroeber had when he began: culture grows; patterns realize themselves, then stop. Was all this labor necessary to support conceptions as general as these? Kroeber tells us at the outset that he wishes to attack his problem "empirically instead of intuitively or *a priori*" (p. 5). At the end, however, he confesses that, although he has "followed a factual route, it ends about where deductive reasoning would have led us more quickly" (p. 789). Unless I exaggerate the implications of this admission, the cost of producing this book in time and labor ("I worked . . . as time was available, from 1931 to 1938," p. viii) gives the publication of "Configurations" an air that is almost tragic. Why pour out your life *via* induction for something that deduction would have given you almost for the asking?

The Boas school's reliance upon induction, their faith that if one can only pile up enough facts one will somehow come to some clear vision and deep understanding,[26] reminds one of Edmund Gosse's childhood faith in numbers, "I was convinced," he wrote, "that if I could only count consecutive numbers long enough, without losing one, I should suddenly, on reaching some far-distant figure, find myself in possession of the great secret."[27]

The fact is, of course, that no amount of mere accumulation of facts will ever produce understanding, at least in the form of basic principles or generalizations of science. As Einstein has well expressed it: "There is no inductive method which could lead to the fundamental concepts of physics. Failure to understand this fact constituted the basic philosophical error of so many investigators of the nineteenth century. . . . We now realize, with special clarity, how much in error are those theorists who believe that theory comes inductively from experience. . . ."[28]

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[26] Boas was forever promising that when enough facts had been gathered, then and not until then would the time come to generalize. "It may seem to the distant observer," he wrote in "The Methods of Ethnology," in 1920, "that American students are engaged in a mass of detailed investigations without much bearing upon the solution of the ultimate problems of a philosophic history of human civilization. I think this interpretation of the American attitude would be unjust because the ultimate questions are as near to our hearts as they are to those of other scholars, only we do not hope to be able to solve an intricate historical problem by a formula" (p. 314).

Of course, Boas never got "enough facts." Besides, "facts" are always too varied, too particular and unique. Consequently at the end of a long and industrious life he was obliged to confess that he had virtually nothing to offer in the way of systematic interpretation (see his Presidential address to the American Association for the Advancement of Science, "Aims of Anthropological Research"). [27] Gosse, 1907, pp. 43–44. [28] Einstein, 1936, pp. 360, 365, 366.

The basic principles of science, according to Einstein, are "free inventions of the human intellect."[29]

There is no substitute for creative imagination.[30]

"Configurations of Culture Growth" is a notable work in another respect, especially in these days, namely, it is a study of *culture*, not of the personality or reactions of the *human organism;* it is, in short, a culturological rather than a psychological study. It was the anthropologists who "discovered culture," according to Kroeber; it was their "distinctive achievement."[31] It was the anthropologists who have realized that culture constitutes a distinct class of phenomena and so requires a distinct science for its interpretation.[32] For decades anthropologists devoted themselves to the study of *culture*. In recent years, however, anthropology—in America at least—has all but abandoned the study of culture *per se* for investigations into the personality and reactions of the human organism. Anthropologists are abandoning culturology for psychology—for psychoanalysis, psychiatry, depth psychology, psychological testing, dream analysis, and so on. This change of objective must be regarded as a regression. As science has grown and matured it has reached successively the following levels: anatomy, physiology, physiological psychology, individual psychology, social psychology-sociology, and finally the science of culture or culturology. The movement away from culturology to psychology, which we are witnessing today in American anthropology, is definitely a regression from a higher to a lower level in the developmental series of strata achieved by science.

This is not to say that psychological studies of primitive peoples should not be made. *Of course they should*. But are anthropologists the proper persons to undertake them? In the first place are they properly trained and equipped to conduct psychological investigations? Apparently they cannot even interpret the results of some of the tests they have learned to administer.[33] Secondly, culture has been the peculiar province of anthropologists. It is they who have discovered culture, they who have developed techniques for its study and interpretation. Anthropologists alone among social scientists are especially qualified to attack problems of culture. If they abandon this study, who is to under-

[29] Einstein, 1934, pp. 33–34.

[30] The "main difficulty of ethnology today," writes Ralph Linton, "lies not in a lack of data but in its uncertainty as to what to do with the material already in hand. The science . . . is weak in its conceptual framework and vague as to its ultimate aims" (Linton, 1938, p. 246).

[31] Kroeber, 1936, pp. 331, 333.

[32] R. H. Lowie has well expressed this view in his essay, "Cultural Anthropology: a Science," 1936.

[33] It may be noted that Ralph Linton, Cora DuBois, A. I. Hallowell and others have relied upon Abram Kardiner and other psychiatrists for interpretations of their data. In the recent study of Pueblo Indian children, "The Hopi Way," it is a psychologist, Dr. Alice Joseph, who does the psychologizing, not Dr. Laura Thompson the ethnologist.

take it and carry it on? Is the study of culture to be abandoned? Is anthropology to sell its culturological birthright for a mess of psychiatric pottage?[34]

In the light of the current trend in anthropology toward "depth psychology" and ink-blot tests, "Configurations of Culture Growth" seems almost old-fashioned, for it is definitely a culturological study. In line with his earlier writings, Kroeber says in "Configurations": "I am convinced that, the phenomenon being cultural, the explanation must first of all be made in cultural terms (p. vii) . . . direct psychological explanations have never got anyone very far in reducing the phenomena of history to order, and I shall not fall back on them" (p. 763).[35] He is not able to hold to this view at every point, however, for we find him wondering if there is not "*something in human nature* which makes men increasingly restive under the unmitigated ban of past generations, until revolt breaks through to overthrow and reconstitute the whole culture" (p. 824, emphasis ours). But this is an exception; on the whole the work proceeds upon a superorganic, culturological level.

In the distinction between culturology and psychology and on the significance of the individual, Kroeber has diverged radically from Boas and many of his disciples. He takes the culturological position which relegates the individual to a role of insignificance or even irrelevance. Boas and many of his followers were never able to catch the vision of a science of culture and, correspondingly, they elevated the individual to a role of great, if not supreme, importance. Kroeber was taken to task on both of these points for his essay "The Superorganic."[36] Sapir demonstrates his inability to grasp the concept of culturology, and objects vigorously to a subordination of the individual.[37] Goldenweiser likewise exalts the individual and discloses his inability to conceive of a supra-psychological science of culture.[38] "The life of culture," he says, "belongs to the psychological level . . . the anthropologist . . . [is a student of] life. Life is psychology."[39] Boas, never able to envisage a science that would study culture as a distinct class of phenomena, pooh-poohs Kroeber's culturological outlook by saying: "It seems hardly necessary to consider culture a mystic entity that exists outside the society of its individual carriers, and that moves by its own force."[40] Ruth Benedict follows suit when she refers to certain persons who "have often expressed themselves in mystical phraseology . . . like Kroeber . . . [who has] called in a force he calls the superorganic to account for the cultural process."[41]

[34] How far this trend has gone is indicated by a recent article in the American Anthropologist. In the opening pages of "The Rorschach Technique in the Study of Personality and Culture," A. Irving Hallowell notes with satisfaction that now, after years devoted to the study of *culture*, anthropologists are at last turning to the study of *people*.

[35] This is the position which Lowie, too, has taken in "The Determinants of Culture" (in "Culture and Ethnology," 1917), in "Cultural Anthropology: a Science," 1936, and elsewhere.

[36] Kroeber, 1917. [37] Sapir, 1917. [38] Goldenweiser, 1917.

[39] Goldenweiser, 1927, pp. 85–86. [40] Boas, 1928, p. 235. [41] Benedict, 1934, p. 231.

To Boas the individual occupied a central position. "The causal conditions of cultural happenings lie always in the interaction between individual and society,"[42] he says. And, by "the working of culture, . . . I mean the life of the individual as controlled by culture and the effect of the individual upon culture."[43] Sapir was an arch-individualist: ". . . it is always the individual that really thinks and acts and dreams and revolts," he insists.[44] His "approach . . . was always through the individual," says Benedict.[45] How far Sapir was from a culturological point of view may be seen from a favorite phrase of his: "There are as many cultures as there are individuals in the population."[46] Goldenweiser declares: "I am an individualist. As such, I place the individual both at the beginning and at the end of the social process.[47] An analysis of culture, if fully carried out, leads back to the individual mind."[48]

Against this Boasian background Kroeber appears in sharp contrast. As in previous articles, so in "Configurations," the individual is regarded as insignificant to the point of irrelevance. This is, of course, a necessary corollary of the culturological point of view in which culture is explained in cultural terms. Cultural phenomena interact with each other on a "superorganic" level and form new combinations and syntheses. The study and interpretation of these events can be carried on not only without reference to individuals, gifted or otherwise, but without reference to the human species. Kroeber does not perhaps go quite this far or make his view and its implications as explicit as we have done here. But this is essentially his position. A man of extraordinary native ability may live and die unknown unless the proper cultural materials and circumstances are at hand to realize his potentialities. Conversely, a certain combination of cultural events might make a man of rather ordinary talents appear to be a genius. As Kroeber puts it, "geniuses are the indicators of the realization of coherent pattern growths of cultural value" (p. 839).

In his adherence to the culturological point of view, Kroeber is not only in opposition to the Boasian philosophy of the past, but to the individual psychological point of view now current in which, as Linton puts it, culture "derives all its qualities from . . . [individual] personalities."[49] "Configurations of Culture Growth" may be the last of a series of important culturological works that began with Tylor's essay on "The Science of Culture."[50] We may now be entering an anthropological Dark Ages of "depth psychology" and ink-blot tests which may endure for some time. But we may be sure, if progress can be expected in the future, that science will again rise to the level of cultural phenomena as a distinct order of reality, and again traverse the road which we can trace from Tylor and Durkheim to Lowie, Wissler and Kroeber.

The works of Kroeber are replete with facts and contain many illuminating

[42] Boas, 1932, p. 612. [43] Boas, 1936, p. 137. [44] Sapir, 1917, p. 442.
[45] Benedict, 1939, p. 467. [46] Benedict, *loc. cit.* [47] Goldenweiser, 1935, p. 75.
[48] Goldenweiser, 1933, p. 59. [49] Linton, 1936, p. 464. [50] Tylor, 1871, Ch. 1.

insights. But, at bottom, he seems to feel that anthropology lacks understanding on some very fundamental points. Two decades ago he declared that "we know as yet as good as nothing of the first or productive causes of civilization."[51] He has expressed similar views elsewhere. In "Configurations" he says that we might answer certain questions if only "we knew *anything* very fundamental about the nature of culture and how it works" (p. 824; emphasis ours). It is not very clear what Kroeber has in mind here, and doubtless if he could put the problem into words concisely, understanding in some degree would be forthcoming as a consequence of the formulation. But there is a specific point in "Configurations" that is pertinent here. When, according to Kroeber, a culture pattern has realized itself, it can go no farther. It will then either stop, or be broken up and a new pattern formed. In the case of Egypt, Kroeber notes the persistence of a pattern for centuries on end. He wonders what it is that would determine whether Egyptian culture would have "become more and more stereotyped and empty *ad infinitum*," or whether it would break out of its rut and develop new forms. It is at this point that Kroeber feels that we know nothing "very fundamental" about "how culture works." It is at this point also that he appeals to "human nature" for an explanation (pp. 823–24).

Might it not be suggested that the motive force of culture is technological, that it consists of energy harnessed and put to work by means of tools and machines? Culture grows as the amount of energy harnessed and put to work per capita per year increases. Increases in amount of energy harnessed have come from the domestication of animals, the cultivation of plants, and from fuels, winds and streams. Improvements in tools and machines increase the efficiency with which energy is put to work, and advance culture as a consequence. According to this view, Egyptian culture could break out of its culture pattern, barring influence from the outside, only if it could materially increase the amount of energy available to it for culture building. Chinese culture has remained relatively constant for a few thousand years because its energy base has not changed. The introduction of engines on a large scale would profoundly alter the configuration of Chinese culture.

These remarks may not meet Kroeber's problem squarely, but they are, I believe, fairly close. We find him dealing with the concept of energy repeatedly in "Configurations," but in a rather tentative and uncertain manner. Sometimes he calls it "ethnic energy" (p. 844), but more often "cultural energy" (pp. 90–91, 842–844). In dealing with change within culture patterns he says: "What the forces are that cause such activity within pattern systems, we do not know. We can call them cultural energy, for want of another label or better metaphor. Cultural energy would at bottom be psychic or physiological activity, but manifested through cultural channels and in cultural forms" (pp. 90–91).

[51] Kroeber, 1923, p. 438.

Kroeber departs from the culturological point of view here. Instead of consistently carrying out his resolution to explain culture in terms of culture, he relapses into an appeal to "psychic or physiological activity." Had he stuck to the culturological level and dealt with energy in terms of the "cultural forms and channels through which it is manifested," namely technology, the arts of subsistence, etc., we believe he would have achieved a more realistic conception of cultural energy. The culture energies of the Arunta, the Chinese (Han dynasty), and the U.S.A. (1945) are not to be understood in terms of "psychic activity" or physiology, but in terms of boomerangs, agriculture, and turbines, in terms of amount of energy harnessed per capita per year.

The interpretation of cultural evolution in terms of technological progress through inventions and discoveries was first set forth in anthropology by Lewis H. Morgan. Tylor also makes use of it from time to time. He speaks of the cultivation of cereals as the "great moving power of civilization," and discusses the culture of England in terms of water power and of steam engines.[52] The present writer has endeavored to reformulate this interpretation in a recent article in this Journal.[53] We find occasional expressions of this view in Kroeber's writings. Thus, in "Anthropology,"[54] he traces the consequences of a mature agriculture: population increase, fixed government, division of labor, etc. Had Kroeber dealt with such basic cultural strata as technologies and social systems in "Configurations" instead of secondary phenomena like music and painting, he might have come to closer grips with a powerful and illuminating concept of culture energy.

In his attempt to bring order out of the "chaotic jumble" of culture Kroeber has, with the exception of his chapter on "The Growth of Nations," confined himself to "the fields of esthetic and intellectual exercise"—music, painting, drama, philosophy, etc. These are not, as he himself states, "the most fundamental cultural activities" (p. 22). He has deliberately refrained from dealing with technological and social systems because he believes that the esthetic and intellectual arts lend themselves more readily and fully to the treatment he wishes to administer than the more basic aspects of culture. It is hardly fair or to the point, perhaps, to take an author to task for not writing the sort of book the reviewer wishes he had written. But we are not "criticizing" "Configurations" in the conventional sense here. We are trying to determine the significance of this work in the light of its historical background, the life work of its author, the present tendencies in anthropology and of probable trends in the future.

We believe that Kroeber would have succeeded in making the growth of culture intelligible to a much greater degree if he had dealt with basic forces and structures rather than with secondary manifestations or expressions. How

[52] Tylor, 1881, pp. 204–205, 215. [53] White, 1943.
[54] Kroeber, 1923, p. 442.

can one reach fundamental interpretations unless one comes to grips with fundamental phenomena? To limit one's study of cultures to music, painting, drama, etc., is like confining the study of vertebrates to fur, feathers, cries, etc. One must get down to the bedrock of anatomy and physiology, to basic structure and process, if results are to be more than superficial. Far from being more difficult to produce results of value on the level of the fundamental, we believe it would be actually easier.

We believe also that Kroeber would have had greater success if he had dealt with culture as a whole instead of tackling it piecemeal. To be sure his problem would have been different had he followed a different course, but I take it that he was not so much concerned with configurations of culture growth *per se* as he was with an objective that he has striven to reach for many years, namely, to make culture in its larger aspects intelligible. We feel that he would have reached a deeper and wider understanding of culture had he grappled with it as a whole. To sketch the growth of the various arts separately is somewhat analogous to tracing the evolution of badgers, butterflies and sweet-peas individually, or even more like trying to work out the evolution of the color patterning of a badger, his claws, and vocal utterances apart from each other. What is needed, in culturology as in biology, is a comprehensive grasp of the field as a whole. We need an outline of the evolution of culture in its entirety, just as we need an all-embracing account of the evolution of life. Kroeber frequently envisions culture as a whole, and he also clearly recognizes its organismic character, as many passages in "Configurations" will testify (pp. 91, 821). Had Kroeber undertaken to trace the development of culture as a whole and in its fundamentals instead of piecemeal and in its superficial aspects, he would have done much to illuminate that "majestic order pervading civilization," about which he had previously written.[55]

Again, let me repeat, this is not said in criticism. To bring order out of chaos, to think in terms of patterns of culture, and configurations of growth—even in such non-basic spheres as music and painting—was a radical departure from the Boas teaching and tradition.[56] As we have seen repeatedly, Kroeber has time and again tried to break away from Boas' domination and to use his own mind freely. The remarkable thing about him, as compared with many other students of Boas, is the extent to which he has succeeded.

A survey of the whole field of science as well as that of anthropology will demonstrate that science is always striving for larger and more inclusive systems of interpretation. Physics is trying to create a unified system that will embrace all physical phenomena, macroscopic as well as sub-microscopic, the

[55] Kroeber, 1917, p. 201.

[56] "No approach is farther than this [i.e., the one followed in Benedict's "Patterns of Culture"] from the customary analytic one of Boas," Kroeber observed in his review of "Patterns of Culture," 1935, p. 689.

theory of relativity as well as quantum mechanics. All life is one; biology is an integral unit. Culture is one, a single *class* of phenomena. A science of culture is needed to grasp and interpret culture as a distinct order of reality, to lay bare and explain that "majestic order pervading civilization." Boas, of course, was vigorously opposed to all this. He had no use for all-embracing systems, broad generalizations or philosophic syntheses. Greater particularity, wider diversity were what impressed him and what he sought for and exhibited.[57] On all of these points Boas was opposed to the spirit and procedure of science everywhere.

The domination of much of American anthropology by such a man for almost four decades has been of course exceedingly unfortunate. To be led by one who was opposed to theorizing, generalizations, syntheses, and philosophic systems, who was a cultural anti-evolutionist, a mind incapable of envisaging a science of culture, could not have been other than disastrous for anthropology. Boas' reactionary outlook and strong personal influence have done American anthropology an injury from which it will be slow to recover.

But recover it will, if we are to have progress in the future as in the past. And when it does, it will again turn to culture as a distinct class of phenomena. Anthropology will be a culturological science rather than a psychological or psychiatric discipline. Culture will be interpreted in cultural terms instead of regarded merely as an "abstraction" or the aggregate emanation of individual personalities. It will deal with culture in its various aspects and time-place groupings, and it will deal with it as a whole. And the science will, of course, trace the evolution of culture from its beginning to the present day so that mankind will, as Tylor remarked, be able, to a degree at least, to forecast the future and to guide our steps toward it.[58] In doing this, however, anthropology will be attempting nothing new. On the contrary, it will merely be carrying on the work begun by such pre-Boasians as Tylor and Morgan, and men like Durkheim who lay outside the Boasian orbit of influence.

When this time comes, "Configurations of Culture Growth" will acquire a significance which it will probably not have now. It will be seen as the major attempt of a man who has repeatedly tried to develop a free and comprehensive study of culture growth. I think it not unfair to say that Kroeber has tried to do what Tylor and Morgan did in a more fundamental and thoroughgoing manner, namely, to grasp culture as a whole and illuminate its principles and processes of growth. Had it not been for Boas' influence, Kroeber would undoubtedly have accomplished more than these men achieved. At the present

[57] "The grand system of the evolution of culture, that is valid for all humanity, is losing much of its plausibility. In place of a simple line of evolution there appear a multiplicity of converging and diverging lines which it is difficult to bring under one system. *Instead of uniformity the striking feature seems to be diversity*" (Boas, 1904, p. 522; emphasis ours).

[58] The closing words of "Anthropology" (1881).

time, it looks as though anthropology in America is in for an era of personality studies and psychiatric analyses. When, however, it returns to its proper sphere and to matters of major importance, such as an adequate interpretation of world culture and of the crisis through which civilization is now passing, "Configurations of Culture Growth" may serve as a point of departure. It gives us *histories* of various cultural growths. A treatise on the growth of culture as a whole would be but one step further.

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BIBLIOGRAPHY

BENEDICT, RUTH
1934 *Patterns of Culture*. Boston.
1939 *Obituary of Edward Sapir*. American Anthropologist, 41: 465–568.
1943 *Franz Boas as an Ethnologist*. (Franz Boas, 1858–1942.) Memoir Series of the American Anthropologist, 61.

BOAS, FRANZ
1900 *Introduction to Publications of the Jesup North Pacific Expedition*, Vol. I, 1898–1900. Memoirs of the American Museum of Natural History, Vol. 2.
1904 *The History of Anthropology*. Science, 20: 513–524.
1920 *The Methods of Ethnology*. American Anthropologist, 22: 311–321.
1928 *Anthropology and Modern Life*. New York.
1932 *The Aims of Anthropological Research*. Science, 76: 605–613.
1936 *History and Science in Anthropology: A Reply*. American Anthropologist, 38: 137–141.

EINSTEIN, ALBERT
1934 *The World as I See It*. New York.
1936 *Physics and Reality*. Journal of the Franklin Institute, 221: 349–382.
1940 *Considerations Concerning the Fundaments of Theoretical Physics*. Science, 91: 487–492.

GERARD, R. W.
1940 *Unresting Cells*. New York.

GOLDENWEISER, ALEXANDER
1917 *The Autonomy of the Social*. American Anthropologist, 19: 447–449.
1927 *Anthropology and Psychology*. (The Social Sciences and their Interrelations; Wm. F. Ogburn and Alexander Goldenweiser, Eds.) New York.
1933 *History, Psychology and Culture*. New York.
1935 *Why I am not a Marxist*. The Modern Monthly, 9: 71–76.

GOSSE, EDMUND
1907 *Father and Son*. New York.

HAEBERLIN, H. K.
1915 *Anti-Professions*. American Anthropologist, 17: 756–759.

HALLOWELL, A. I.
1945 *The Rorschach Technique in the Study of Personality and Culture*. American Anthropologist, 47: 195–210.

KLUCKHOHN, CLYDE
1943 *Bronislaw Malinowski, 1884–1942*. Journal of American Folklore, 56: 208–219.

KROEBER, A. L.
1917 *The Superorganic*. American Anthropologist, 19: 163–213.
1918a *The Possibility of a Social Psychology*. American Journal of Sociology, 23: 633–650.

1918b Review of *The American Indian* by Clark Wissler. American Anthropologist, 20: 203–209.
1919 *On the Principle of Order in Civilization as Exemplified by Changes of Fashion*. American Anthropologist, 21: 235–263.
1920 Review of *Primitive Society* by R. H. Lowie. American Anthropologist, 22: 377–381.
1923 *Anthropology*. New York.
1935 Review of *Patterns of Culture* by Ruth Benedict. American Anthropologist, 37: 689-690.
1936 *So-called Social Science*. Journal of Social Philosophy, 1: 317–340.

LINTON, RALPH
1936 *The Study of Man*. New York.
1938 *The Present Status of Anthropology*. Science, 87: 241–248.

LOWIE, R. H.
1917 *Culture and Ethnology*. New York.
1920 *Primitive Society*. New York.
1936 *Cultural Anthropology: a Science*. American Journal of Sociology, 42: 301–320.
1943 *Obituary of Franz Boas*. Scientific Monthly, 56: 182-184.

SAPIR, EDWARD
1917 *Do We Need a "Superorganic"?* American Anthropologist, 19: 441–447.

THOMPSON, LAURA, AND ALICE JOSEPH
1944 *The Hopi Way*. Chicago.

TYLOR, EDWARD B.
1871 *Primitive Culture*. London.
1881 *Anthropology*. New York.

WHITE, LESLIE A.
1938 *Science is Sciencing*. Philosophy of Science, 5: 369–389.
1943 *Energy and the Evolution of Culture*. American Anthropologist, 45: 335–356.
1945 *History, Evolutionism and Functionalism: Three Types of Interpretation of Culture*. Southwestern Journal of Anthropology, 1: 221–248.

BOOK REVIEWS

NORTH AMERICA

The Long Journey. JOHANNES V. JENSEN. (Translated from Danish by A. G. Chater. 667 pp. $3.50. Knopf, New York.)

This book, issued as the Nobel Prize edition of a well-known work, first published in Denmark and shortly after in translations in both Germany and this country all of twenty years ago, now necessarily comes in for some well-deserved second thoughts. Awarded the 1944 prize "for Literature," it has naturally received most attention from the literary spokesmen. In fact, this reviewer knows of only two fellow anthropologists who have even so much as looked at this, perhaps the most formidable attempt to give us at once a readable and plausible account of human culture history. Under the circumstances it seems imperative to consider briefly not only the performance itself, but the author's peculiar fitness for the task, as well as the background leading to his conception.

First then as to background. Fictionized history in the form of the historical novel having long been widely popular, it is easily understandable that in recent decades many and varied attempts should have been made to arouse our latent interest also in fictionized prehistory. This "latent interest" at once serves to relegate any suggestion of commercialism to its proper secondary place. The more obvious shining fact is, as is abundantly proved by the welter of creation myths available from all parts of the world, that the human imagination has been busy with practically all questions of origin from time immemorial. Up to less than a century ago these more or less suggestive explanations of things as they are, were, as far as we can now see, pure speculations. Since then, however, prehistoric archaeology has unearthed a considerable body of substantial facts about the early hitherto unknown life of man; and these facts, like the somewhat less dry facts of history and biography, are now in process of being dressed up in more attractive form. The result is a literal attempt to put life into dry bones, a sort of embroidering or sugar-coating perhaps, but if historians and biographers allow such transmutation of their material, then why should anthropologists object to the method being used also for imparting the essentials of prehistory?

As intimated, during the present century many have tried their hands at this type of fiction. Naturally, therefore, the output varies both in its intended appeal and in intrinsic quality. Some experiments, like Jack London's *Before Adam*, are strong on fiction, while, by contrast, the numerous contributions of Hendrik Van Loon are equally strong on facts. In the present author we have a fair balance of the two tendencies. Johannes V. Jensen has long been recognized as a prolific and versatile literary artist, but, equally important for us is the fact that in preparation for an unfinished medical course he was permanently inspired by the teachings of one of Europe's outstanding systematists in vertebrate zoology. The resulting interest in animal creation, rounded out by extensive travel studies and wide reading, not only in natural history but also in archaeology, folklore, surviving customs and superstitions, as well as in formal history and biography, has made him perhaps uniquely competent to delineate the slowly unfolding story of humankind. Jensen, let it be known at once, is an evolutionist, for although he rarely uses the word, he leaves no one in doubt that the idea of

growth, of continual becoming, is the core of his philosophy, perhaps of his religion. As if to clinch this for us, he published in 1927 a book (probably not translated) entitled *The Transformation of Animals*, a book which in reality is a necessary introduction to a full appreciation of the work here to be considered. No reader of this delightfully illuminating volume is likely to lay it down without first being carried away by the notion of the perfect naturalness of the biological origin, not merely of the lower primates, but of man himself.

At this point then *The Long Journey* takes up the narrative. Man, in recognizable form, having been accounted for as the end result of eons of physical transformation, the author now proceeds to sketch in broken outline this creature's mental development as revealed by the archaeological and historical records. In other words, we are still concerned with man in process of transformation, and this time the exposition is presented, not in essentially scientific form, but instead as a poetically conceived adventure, a prose epic as it were, of mankind.

Originally issued as six separately titled books, here admirably translated and presented (with unimportant omissions) as a single compact volume, the work conducts the patient reader up through the successive millenniums from pre-glacial and all but pre-human times to the discovery of America by Columbus! Specifically, each book describes the natural and human scenes at some particular stage of cultural development, and the drama—in reasonable agreement, it would seem, with the Carlylean concept of progress—is supplied chiefly by the inventive activities of the contemporary guiding genius and his struggle with the normally conservative group. Thus in the first book our culture hero introduces the use of fire and invents the first simple implements; in the second (untold centuries later) he devises, among other things, a method of producing fire by striking sparks from two pieces of flint; and in the third he appears as boat-builder, navigator and explorer—the forerunner, we may suppose, of Columbus. Near this point the tale rises to the historic and biographic level and so passes beyond the prehistorian's immediate interest; but, briefly, the episodes dealt with concern the little-known wanderings and tragic fate of the Cimbrians of about 100 B.C., the more or less notorious viking raids and conquests of roughly a thousand years afterwards, and, finally, the late well-known exploits of Columbus and some of his followers in America.

Granting the epic character of the work as a whole and the suggestive quality of its various dramatic details, the perplexed reader may still ask: what, precisely, is it that holds the separate parts together? The scientifically minded may be expected to answer that the idea of evolution is the all-pervading theme, but to this, one suspects, the artistically minded will hardly agree. They may insist instead on something less obvious, as, for instance, the unknown force that makes evolution move. As if himself uncertain, the author has published at least two follow-up books, one called *The Stages of the Mind* and the other *Esthetics and Evolution*, the latter of which is said to explain his unifying conception. These, unfortunately, have not been read. But, concentrating simply on what we have, the thoughtful reader must have noticed the author's repeated suggestion of humanity controlled, like the Wandering Jew, by some cosmic urge, inner drive, or restless longing for a return, as he puts it, to the vaguely remembered "Lost Land," i.e., the ancestral jungle. This impelling force, taken literally and again in his

own words, drives sometimes to progress and sometimes to destruction—a state of affairs scarcely neither more nor less mysterious than the migratory movements of birds, mammals and fishes, or the persistent flight of insects towards the light. An expert in teleology perhaps is indicated. Whatever the verdict, a work of art is primarily an adventure in esthetics rather than in either logic or science, an appeal to the heart as well as to the head, and is entitled to be judged accordingly. But how? In evaluating a piece of embroidery some connoisseurs might insist on the importance of the warp-and-woof foundation, while others would see only the applied decorative pattern.

Under these complicated circumstances, pointed criticism of *The Long Journey* becomes a trifle precarious. At any rate, and oddly enough, the literary commentators who have charged about seem inclined to single out questionable scientific assumptions rather than merely artistic peculiarities. They might well have dwelt on the somewhat difficult style, the strange absence of dialogue and, above all, on the introduction of mythical and legendary actors—a touch of the miraculous which leaves the literalist dumbfounded. Instead, some of them think the author has gone wrong in having man originate in Sweden rather than in the tropics. They might better have objected to his emergence near a "volcano" that never existed. The rocky promontory in question is doubtless visible from near Jensen's summer home in Denmark and so, perhaps in a generous mood, he chose it as his starting point. His human precursors of that place expressly originated in the tropical forest which at the time extended north of the Baltic shores. Later on, however, as glacial conditions approached, most of these lowly creatures, reasonably enough, are imagined to have retreated south together with their jungle habitat, while the few who chose to remain behind, by somehow adapting themselves to the harsher new conditions, became truly human and as such began laying the foundations of modern civilization, a phenomenon until recent times confined chiefly to the north temperate zone. The locale of this first step, it is true, might better have been referred to England or to China, but, for all we know, it could just as well have been in Sweden.

Finally, there are hints also that our author is a champion of Nordicism. Such a pose, if indiscreet, is still not a crime, but the charge is certainly not borne out by Jensen's other writings. It must be admitted that this particular work starts out as an epic of humanity and ends up essentially as the saga mainly of members of the author's own racial group. Possibly the historical part was originally done for home consumption and, like some of the strictly local scenes, should have been left out of the translation altogether. But, by way of extenuation, it seems not unlikely that a sensible Chinaman, attempting a similarly coherent account of human progress, would have limited his considerations largely to the Middle Kingdom. In any case, Jensen, without apology, began his observations over forty years ago in his own front yard, but he did not fail to look beyond the fence, and something of what he saw there in the way of vaunted racial and cultural superiority gave him due pause. Thus in 1927 when writing, incidentally, about the over-specialized saber-toothed tiger and his consequent extinction, he ends up with the now pertinent remark that "certain modern overgrown military states remind us . . . of this monstrosity" and, by implication, of his fate. No evident admiration of demonstrative superiority here! Indeed, as an evolutionist, Jensen, while of course intensely interested in nature's past mistakes in specialization,

emphatically concludes that the one chief lesson thus taught us by the past is, in substance, that in the long run "the meek shall inherit the earth." And yet, curiously enough, for actual progress he insists on the need of the Culture Hero, usually, as in the case of modern dictators, to be sacrificed if he carries his program too far.

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The Navaho Door, An Introduction to Navaho Life. LT. ALEXANDER H. LEIGHTON, M.C., U.S.N.R., AND DOROTHEA LEIGHTON, M.D. (Foreword by John Collier. 149 pp., 36 pls. Harvard University Press, Cambridge, 1944.)

The "Navaho Door" is symptomatic of a new era in social research—the collaboration of different disciplines in the understanding and solution of problems that formerly would have been considered the field of investigation for only one of them. In this case, anthropology (most particularly in the person of Dr. Clyde Kluckhohn) has furnished the medically trained Leightons with a comprehensive picture of Navaho life. They, in turn, have found new values in many well-known features of Navaho culture.

The Leightons' chief concern, as might be expected from their specific experience, is with matters of health, their chief task to persuade a minority and largely illiterate group who have numerous preconceptions regarding the nature and treatment of disease to accept the professional advice of the White "medicine man." Great importance is placed on having a good interpreter, on the need for making everything clear to the sick man and to his family. As practising physicians, the Leightons have learned that adequate explanation is not only the first step in overcoming individual prejudice and distrust, but an important device for implementing general programs of cure and prevention.

The chapters entitled "Navaho Ways and White Man's Medicine—in the Hospital" and "Navaho Ways and White Man's Medicine—in Out-Patient Work" contain many illuminating observations and wise suggestions. Navaho attitudes toward religion and disease are carefully analyzed. Navaho familiarity with the sweatbath, with ointments, herbal concoctions, inhalants, emetics and cathartics becomes the basis for extending a knowledge of strange medicines and methods.

It is this recognition of the positive values inherent in Navaho experience, as well as an insistence on the dignity of the Navaho as a human being, that inspire some of the Leightons' most original contributions. From the standpoint of the usual hospital routines, it may seem ridiculous to permit a close relative to attend a very sick patient just to temper his loneliness in an alien environment, or to permit a nursing mother to accompany her sick child so that a feeding schedule will not be interrupted, or to respect a native woman's modesty in such matters as bathing or examination, or to ask a native medicine doctor to act as mediator between hospital and patient or give a short curing rite at the latter's bedside. But these are some of the procedures recommended by the authors to facilitate Indian acceptance of White practice. The imaginary interviews between trained physician and native patient regarding ailment and treatment further illustrate their constructive work methods.

As John Collier has pointed out in his introduction, the implications of this book "are wider than the area of health alone." Indeed, in this changing world, an acquaintance with the Leightons' approach and field techniques can be of inestimable aid to educators, social workers and administrators, no less than to anthropologists who hope to foster better understanding between groups of widely different habits and traditions.

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Navaho Witchcraft. CLYDE KLUCKHOHN. (Peabody Museum Papers, 22, No. 2. 1944.)

This interesting volume develops two themes of different value (used in the artist's sense of intensity of color) and aimed at two or more separate audiences. The first, which, though it appears only in Part II, seems to be given the most weight, is aimed at the psychologist and, perhaps, the sociologist. We do not mean to omit the anthropologist but only to imply that this audience is a wider one, with a different kind of technical interest. The gist of the theme is:

The Navaho, since their captivity at Fort Sumner, have been in a state of growing insecurity, which finds its outlet in a belief in witchcraft. This belief helps to maintain faith in the ceremonials by explaining their occasional failure. It allows individuals to take the center of the stage by announcing themselves as victims. It provides an outlet for hostile impulses which cannot be given full rein against family, neighbors or the government. It endows life with drama, permits the verbalization of anxiety, and keeps agitators under control through fear that they will be suspected as witches. Finally, it affirms the individual's unity with the social structure, since the curative chant involves the whole community. An interesting point for the student of acculturation is the increasing emphasis on economics. Witchcraft is most often practised against the rich, by those who envy them.

This discussion of "cultural dynamics" will be full of suggestions for those wishing to look more closely into the social functions of any belief which they find flourishing. It could easily be amplified to apply to a number of primitive groups and a number of beliefs. In fact, the reviewer would like to see it so treated and applied even to the Germans whose need for a scapegoat, as Dr. Kluckhohn truly remarks, is only one step removed from that of the Navaho.

A theme of this sort, for hammering home its point, does not need any description of the concrete forms of witchcraft. Such details might even distract the mind of the reader by leading him to seek associations and emotional reasons for the different concepts. The concrete data form, actually, a second book, aimed more particularly at the anthropologist. Yet not entirely, perhaps, for this portion describes *ab ovo* and renames forms of magic long current in the area. These are:

The use of body refuse to make a medicine which can kill. This Dr. Kluckhohn calls "witchery." It is practised by a group of people who meet at night, using songs and prayers. They make body leavings harmful by placing them in contact with the dead and they do actual murder in the shape of were-animals. The best preventive against them is a medicine made of the galls of prey animals.

2. The misuse of chants by a singer who employs the sacred formulae to kill instead of cure. He can order snakes or lightning to do his work or can operate through body

leavings like the witches. Dr. Kluckhohn elects to call him a "sorcerer." The remedy against him is a chant.

3. Shooting a disease object into the body. This is called "wizardry" and the remedy is the service of a sucking shaman, usually found in some other tribe.

4. Eating certain plants which, in connection with the chant, Prostitution Way (could we not get rid of that moralistic western title?), can bring game or women to the practitioner. They can also overcome an enemy in games, destroy his flocks or spoil his hunting luck.

These statements appear amplified in the Appendix, along with the field notes, *in extenso* and in the English of Navaho informants. This is a brand new alignment of material and, expectably, it involves some blurring and omissions. The theoretical portion, as we have implied, is a pathfinding statement of general principles, which might be developed in a separate volume. After that, some paragraphs and a reference would be enough. The concrete portion, on the other hand, has to divide its energies between telling the new readers what a sucking shaman is (heretofore we have avoided the word sucker) and giving the details of the Navaho situation. To the reviewer's mind, these details are insufficient. A vague surmise, repeated a dozen times by informants, does not add to one's knowledge. For this reader, such material might better be summarized and its space given to case histories as a background for the few connected events mentioned. What were the relations of the people concerned? Who was rich and who was poor? Who was the narrator and what was the function of witchcraft belief for him?

Moreover, if we are going into witchcraft methods at all, can their importance be evaluated without comparative data? For instance, the witch society is rated number one on Dr. Kluckhohn's list because informants mentioned it so often. If this belief had been evolved by the Navaho, despite the fact that their organization provides no model for it, we might credit them with a strong belief in the solidarity of their enemies —perhaps, conversely, with a sense of their own lack of solidarity. The situation changes when we know that Ramah, scene of Dr. Kluckhohn's studies, is neighbor to Zuni, where the witch society is an old belief. It then becomes our interest to inquire how far the Navaho have really ingested this belief, adding elements of their own. The reported *yebichai* masks and the emphasis on contact of witches with the dead may be such elements. Are they not as germane to the subject as an attitude toward flock reduction?

The use of an appendix such as this, with its verbatim field notes, would provide more material for discussion and involve, perhaps, an additional group of readers, the linguists. We leave these details of technique to be worked out in—we hope—many different ways as the old standard form of anthropological monograph gets a complete overhauling.

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The North American Indian Today. C. T. LORAM AND T. F. MCILWRAITH, editors. (xi, 361 pp. $3.00. University of Toronto Press, Toronto, 1943.)

Owing to the sudden death of Professor Loram a year following the Conference, the full onus of editorial responsibility for the papers fell upon Professor McIlwraith.

The sheer multiplicity and variety of the papers put a tremendous burden of integration on any editor, if they were to be published in meaningful form. This reviewer, for one, admires the job McIlwraith did.

Although one cannot review thirty-odd papers, one can ask, "What does the brunt of them accomplish?" First of all, McIlwraith's preliminary statements running through the book do insure the reader, unacquainted with Indians and Indian administration, against leaving the book at the end with no over-all ideas about the setting of the "Indian problem." Secondly, those of us who have some acquaintanceship with things Indian can pursue profitably the individual papers by studying the viewpoints implied as well as expressed, in the light of the occupation or profession of the person who delivered the particular paper. For example, if one wants to see a justification of a huge monopolistic trading enterprise with natives, one can read the excellent statement by R. H. G. Bonnycastle of The Hudson's Bay Company. In the same way the missionary viewpoint is well portrayed, with varying nuances between Catholic and Protestant, in the section devoted to "The Indian and The Missionary." The Reverend J. B. Tennelly's statement in this section, by the way, is an admirable presentation of the Catholic viewpoint. And so one could go on with John Collier's paper on the policy he inaugurated in the U. S. Office of Indian Affairs. We get a positive statement of the policy and its good points that very few people question, but little idea of its far-reaching implications, both good and bad.

There is thus much value to be harvested from these papers by treating them as documents of the time. Some of them in addition are valuable in themselves, both for the information they give as well as for approaches to a problem that still remains a knotty one.

It is good that we have a record of this Conference in published form. But probably the main value of the Conference was the getting-together of men who were active and influential in some capacity with Indians, so that they could exchange their experiences and points of view with each other—missionaries, government administrators, traders, anthropologists, physicians, Americans and Canadians.

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Malaria in the Upper Mississippi Valley 1660–1900. ERWIN H. ACKERKNECHT. (Supplements of the History of Medicine No. 4. viii, 142 pp., 5 maps. $2.00. The Johns Hopkins Press, Baltimore, 1945.)

This excellent study in medical epidemiology should be of interest not only to malariologists but to all who have any interest in the opening up and conquest of the West. The "ague," as malaria is still commonly called in many parts of the United States, was a far worse enemy of the white settler than the legendary Indian was ever supposed to be. In the present monograph Dr. Ackerknecht is not, however, concerned with that story, but rather with the medical history of malaria in one section of the United States, the upper Mississippi Valley. This takes in the states of Illinois, Missouri, Iowa, Wisconsin, and Minnesota. From these states malaria has today practically disappeared. Dr. Ackerknecht discusses the known and probable causes for this decline and in a final section, "Some Remarks on Malaria and History," he enlarges upon

the relevance of his findings for the theory of the decline of certain ancient civilizations as a result of wholesale malarial infestation. Though brief, this is an important section, for as the author shows, even under unfavourable conditions "a highly malarious region might march forward towards progress almost unimpeded by malaria." Malaria cannot therefore be regarded, as it has been by some authors, as a sufficient cause of the decline of certain civilizations.

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COLD SPRING HARBOR, LONG ISLAND, N. Y.

Tenants of the Almighty. ARTHUR RAPER. (x, 403 pp., illus. $3.50. Macmillan, New York, 1943.)

What happened to the farmers, Negro and White, of Greene County, Georgia, in 1934 and 1935 when the various federal agencies moved in to help the poverty-stricken tenantry, with historical background that goes back to the Indians. A good case study of a southern rural community, though the style, which is what an academic man does when he tries to write a "popular" book, makes the reading rather trying. Excellent photographs by Jack Delano, and some useful statistical appendices.

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Observations on the Yurok: Childhood and World Image. ERIK HOMBURGER ERIKSON. (University of California Publications in American Archaeology and Ethnology, 35, No. 10. vi, pp. 257–302. $.50. University of California Press, Berkeley and Los Angeles, 1943.)

Erikson brings to his study of the Yurok a rare combination: psychoanalytic insight, an artist's selectivity and perception of basic composition, and an understanding of what the anthropologist means by "culture." The results are quite apparent.

One can follow through the monograph with a little symbol in his mind's eye: an ellipse bisected longitudinally by a line of movement flowing in one direction but constricted in the middle of the ellipse. If one added to this symbol, as a threat, a movement in the reverse direction at the base or top of the ellipse, one would have a complete and dynamic symbol that could be applied to the Yurok world.

There is a fairly rare phenomenon called *cardiospasm*, which is a closure of the alimentary tract above the stomach. It seems to be psychogenic. One might speculate on the possibility of whether such a phenomenon among the Yurok would be found in greater or less proportion than among us. The cardiospasm idea runs through the culture, from the salmon obstruction in the middle of the river to the constricted opening at the back of the men's houses. It would be, in any case, an interesting thing to investigate.

Erikson has uncovered the operations of this symbol in the religion, folklore, daily life, houses, and, to be brief, in the whole culture of the people. In spite of changes in the culture the Yurok conception of the world, their world, still lives on, strongly and explicitly in the old people and implicitly in the young through their child training: "... whatever of the past is still part of his child training."

In these American Indian situations one must ordinarily work with drastically changed cultural conditions. The Yurok have some continuity with the past in their economic rhythm of life and in the salmon. Also their concept of property is close to that of the American white. However, even with the Yurok, if one is interested in the original culture, one has to practise "archaeology" on the minds of the old people, and dig out what once was. This information is subject to distortion, as we know. However, a valid check is to be had through the psychological configurations that persist long after the manifest culture has begun to fade out. This Erikson has appreciated and used.

Erikson's psychoanalytic psychology belongs to that stage of interest in the "libidinal" approach and in the erogenous zones. It is sensitively employed and produces results. The reviewer wonders what kind of picture of the Yurok would have emerged had the later "ego adjustment" approach of certain psychoanalysts been used. The questions asked of the material, of course, would have been different. "How in this particular total environment does the individual adjust himself and what personality configuration emerges?" would be a central question for the "ego" school. This on the whole is a more fruitful approach. It is used for instance, by Kardiner and Linton. However, it is difficult to apply to a situation in which many aspects of the culture have changed rapidly—unless one is interested in the contemporary situation primarily.

For insights this latest by Erikson is fertile, as is his thinking generally. I like his footnote on page 299, part of which I shall quote: "Advocating progress and rationality rather than tradition and belief, all-embracing universality rather than rigid exclusion, civilization endangers the remnants of tribal synthesis for the sake of an unknown future standard for all. The revolutions of today seem to represent attempts at consolidating human gains somewhere midway, with race or class replacing tribe, and world domination or world revolution replacing universality. The fate of the democratic and rational education will depend on the degree to which rationality will make for a new and more universal cultural homogeneity."

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MIDDLE AND SOUTH AMERICA

The Origins and History of the Mayas. M. WELLS JAKEMAN. (Pt. 1, xxv, 203 pp., tables, maps, diagrams, illus., bibl. Research Publishing Co., Los Angeles, 1945.)

Most archaeologists now or recently active in the field of Maya research feel that no broad reconstruction of Maya history can achieve the status of a working hypothesis until the results of late researches which have tended to reverse completely their attitudes toward important issues have been properly evaluated, and the more serious gaps in our information have been substantially reduced. This is not so much because all previously advanced reconstructions are failing as because the uncritical acceptance of certain broad propositions based on incomplete evidence has in several instances in the past impeded discovery and interfered with an impartial appraisal of available material. There has been a growing tendency to distinguish sharply the propositions

which are offered as working hypotheses from those which are largely speculative, and whose chief value is in their collective potentiality to stimulate imagination, suggest new ideas, and reveal fundamental issues. It is difficult to know how best to approach such a work as Mr. Jakeman's, as outlined in the introductory volume, for though its primary object is a broad reconstruction based largely on documentary studies, the foundation which Mr. Jakeman lays for this structure in his introductory volume, and also in the volume to follow, which is devoted to special interpretative investigations, far surpasses in depth and scope the dimensions of a purely speculative work. The first volume amply demonstrates Mr. Jakeman's superb talent for the systematic organization of material, which enables him to include an incredible amount and variety of it in a book of less than two hundred pages without marring his essentially lucid and pleasant style of exposition. The general introduction is particularly good in this respect, giving a simple outline of his projected work and stating its relation to the wider field of Maya research, of which Mr. Jakeman sketches a bird's-eye view, at once comprehensive and accurate.

In contrast, the first chapter, which reviews archaeological evidence, fails to appraise correctly the present status of archaeological opinion. It is entitled "A Summary of the Present Archaeological Reconstruction," but does not give even a hint of the fact that no such reconstruction has ever been accepted without reservation, and that it is now in the process of being revised by at least one group of archaeologists who are holding their judgment in abeyance pending the fuller publication of recent researches temporarily suspended by the war. These researches have already been announced by Dr. Pollock, Dr. Brainerd and Dr. Andrews in the Carnegie Institution year books of 1935–44. Mr. Jakeman does not, however, include the results of these investigations in his summary of evidence, and apparently is not aware how radically they change the archaeological picture. Specifically, this leads him to designate the late, elaborate phase of Puuc architecture as "early stage in central and northern Yucatan," in the face of such statements as the following: "Perhaps most interesting of the season's finds was a group of sites indicating widespread architectural activity in northern Yucatan in early Old Empire Times" (E. W. Andrews, C. I. W. Year Book, 1941–42). "In addition, a re-examination of the long-held theory that the major cultural development of Yucatan coincided with the decline of the Old Empire has been made advisable by the increasing evidence that Yucatan supported a considerable, widespread, and culturally advanced population during and before the time of the Maya Old Empire, . . ." (G. W. Brainerd, C. I. W. Year Book, 1941–42). The omission without adequately stated reason of such sites as Santa Rosa Xtampak on the Old Empire horizon and of Yaxuna in the early period, which in Mr. Jakeman's scheme is represented only by Uaxactun, with a qualified inclusion of Tikal and Balakbal, is so serious a flaw that it renders his archaeological reconstruction almost completely useless as a basis of argument, and perhaps makes superfluous the minor criticism that Mr. Jakeman depreciates unduly the importance of the distinction between the early and the late phase of the so-called "Old Empire," which is strongly evidenced in art and architecture as well as in ceramics.

It is regrettable that archaeological publication, especially now, lags far behind discovery, but since the opinions of archaeologists are based on a larger body of data

than is available in publications, a worker in another discipline might profit by consulting these opinions before he attempts to construct a summary which purports to reflect the present status of research.

The succeeding chapters of Mr. Jakeman's book are almost entirely devoted to documentary studies. I prefer to leave specific criticisms of these chapters to experts in that field, but I find it difficult to make a general comment that would serve to introduce them to archaeologists, since they vary widely in content and presentation, and consequently in the interest they hold for the archaeologist. The second chapter is a general survey of documentary sources. The third is a brief exposition of the Maya calendrical system and an attempted appraisal of the calendrical correlations proposed. Many archaeologists will agree with this appraisal, but will feel that the result is stated too decisively in view of the uncertainties within the tests applied. There follow some illustrative texts, and Mr. Jakeman's original reconstruction of what he calls the "Historical Recollections" of Gaspar Antonio Chi, as evidenced by their incorporation in the *relaciones* of the *encomenderos* of Merida. Chapter 5 will interest archaeologists, since it deals with the authenticity and reliability of documentary sources. It will not entirely lull their suspicions concerning the Katun chronicles of the Books of Chilam Balam, but it may help them substantially to clarify their attitudes. What has always bothered the archaeologist is not so much whether they are trustworthy, as how they should be interpreted. In Chapter 6, Mr. Jakeman presents at length a systematized method of historical interpretation. At first the archaeologist will be inclined to skip this chapter, which does not concern him directly and is highly technical, but after reading the seventh, in which the method is applied to the Katun chronicles, he will be prompted to examine with some care the remarkable mechanism which seems to produce such startlingly radical results. He will regret that there is no preface summarizing expert opinion as to the validity of such methods in general, to which only a bibliographical reference is made, and will await the verdict of those competent to judge.

My general impression of these chapters is that while Mr. Jakeman has taken pains to strengthen with intricate procedures some of the propositions which lead to his conclusions, on others he has cut corners with what seems like unwarranted insouciance. For example, when he is preparing to test correlations by their accord with the historical information of the chronicles he simply states, speaking of the u Kahlay Katunob, "whose dates of historical events are placeable in the European Christian Era independently of the calendrical correlation problem (by reason of *the fact that they are recorded in a continuous sequence . . .*)" (italics mine). If one turns to the illustrative translation on page 65, it is obvious that this is true only after the translator has inserted missing Katuns and made a correction in the original compiler's calculations. The burden of proof seems to be on the translator, for statements 2 and 3 could be construed to be a repetition of the description of the same event copied from two distinct sources and somewhat differently worded. The archaeologist is not disposed to accept this conclusion on authority alone, until there is substantial agreement between workers in the field of documentary research on the true sequence of these Katuns and the intention of the chroniclers.

Viewing the volume as a foundation for a broad reconstruction of Maya history it seems to have a certain disproportion of emphasis. Parts of it are buttressed with ponderous procedures, while others, equally important, totter without visible support.

From this viewpoint it would seem that the work would gain balance if Mr. Jakeman could spread his critical appraisements more thinly, perhaps, but more evenly over the whole of the structure. On the other hand, his special researches which he subordinates to the main object of reconstruction deserve more detailed treatment, and in this light the disproportions seem to be due to the attempt to combine two objectives, each having very different requirements for success. A general reconstruction should be presented with the expectation that it will become obsolete in a relatively short time and may have to be demolished to make room for another. The danger is that a rejection of the general reconstruction may prejudice the evaluation of the specific results he will have achieved, and that logical structures that otherwise may have stood on their own will tend to be pulled down in the general demolition.

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The North Highlands of Peru, Excavations in the Callejón de Huaylas and at Chavín de Huántar. WENDELL C. BENNETT. (Anthropological Papers of the American Museum of Natural History, 39, pt. 1, 114 pp., 8 pls., 33 text figs. New York, 1944.)

This report is another landmark in Andean prehistory. Following in the tradition set by the North American Peruvianists, Uhle and Kroeber, Bennett conducted limited but well-planned test excavations in the North Highlands of Peru over a five month period. Like his predecessors, he has drawn upon a background of comprehensive knowledge in Peruvian archaeology to organize and integrate his findings. The data have been expertly analyzed and the evidence effectively marshalled to give the first understandable picture of sequential archaeology in the Callejón and upper Marañon region.

Bennett is to be commended for making strategic excavations in terms of problems rather than simply throwing out the archaeological "drag-net" to see what manner of strange fish would turn up. Previous researches and a study of collections indicated the association of Recuay and Tiahuanacoid styles in the Callejón de Huaylas. As Bennett and others had already demonstrated, both Recuay and Tiahuanacoid have important sequence implications on the North Coast of Peru; and Tiahuanacoid influences mark the widely diffused horizon style of the pan-Andean Middle Periods, a basic reference datum in Peruvian chronologies. By inference from the North Coast, it had been thought that Recuay probably preceded the North Highland Tiahuanaco variant, but nevertheless it remained as a point of chronology to be demonstrated. A second problem of equal importance incorporates a series of questions centering upon the famous site of Chavín de Huántar. At the time of Bennett's investigations in 1938, Chavín ceramics were known on the North Coast where their classification as such depended upon their resemblance to the stone carvings from the highland name-site. The pottery of Chavín de Huántar was still virtually unknown. Furthermore, the time positions of Chavín, and Chavín influences, with relation to all of the Peruvian horizons, were very ambiguous. The obvious thing was to excavate at Chavín de Huántar, and Bennett proceeded to do so.

The successes of the Callejón and Chavín excavations can be summed up very simply: (1) The classic Recuay style was established as antecedent to the Callejón variety of Tiahuanacoid (Wilkawain-Tiahuanaco). (2) The ceramic composition of Wilkawain-Tiahuanaco was determined, confirming this local Callejón period as a part of the major Peruvian Middle Periods. (3) Important architectural features were found definitely associated with both the Recuay and Wilkawain-Tiahuanaco styles. (4) The ceramic type of Chavín de Huántar was established.

In addition to these gains, which came as answers to questions formulated in advance, new problems arose from the digging which will serve as bases for future investigations. One of the most puzzling of these has to do with the ceramics at the Chavín type site. This pottery is not in the specific style of the associated stone carvings or of the ornate North Coast Chavín (Cupisnique) ware. Curiously, its nearest affiliates are the relatively simple incised wares of Early Ancon and Early Supe of the Central Coast. This discovery (of which colleagues had been apprised previous to the publication date) has led into a general re-appraisal of Peruvian cultural chronologies, strengthening Tello's early claims for a wide-spread Chavín horizon underlying the Early Periods. Unfortunately, in this connection Bennett was unable to place Recuay and Chavín in stratigraphic relationship to each other. Chavín ceramics are yet to be discovered in the Callejón, and no Recuay materials were found in the vicinity of Chavín de Huántar. A second new discovery with interesting implications for the future was the appearance of White-on-red style ceramics in intrusive graves at Chavín and in an isolated grave near Huaraz in the Callejón. Bennett cautiously suggests a relationship to Chancay White-on-red (Early Period) and to the Salinar (post-Cupisnique) style of the North Coast. The time position of all of the periods involved is favorable to such a correlation, and the reviewer has already committed himself to the thesis that these White-on-red finds of Bennett's are but a regional manifestation of a pan-Peruvian White-on-red horizon style.

Throughout the report, which treats in detail of, first, the excavations in the Callejón near Huaraz, and, second, those at Chavín, the reader cannot help but be impressed at the sureness and skill in excavation technique. Here is no whisk-broom and orange-wood stick artistry devoted to neatness as an end in itself, but an ingenious adaptability to meet each field situation. Bennett was confronted with multi-storied stone temples, above-ground and subterranean stone-lined houses, deep tombs, subterranean galleries, and combined rubbish-structure conditions. Through it all he maintained a good balance between the primary objective of sequence relationship determination and the ever-constant objective of careful recording of structural features. Such is no small task when one is confined to test-pit samplings. The gallery-house stratification in the Callejón and the correlation of Chavín architecture with the incised ceramics of the refuse Chavín de Huántar are models of craftsmanship in field work.

I am inclined to disagree with Bennett's pottery analyses. This criticism is from the point of view of method, not style identifications. Bennett's procedure has been to use the style category, such as "Recuay" or "Wilkawain-Tiahuanaco," which covers a considerable range of variability, and then to subdivide the style into exceedingly numerous descriptive groups. These subdivisions, or "types," are based upon vessel-form variation, and, in addition, descriptive data as to surface treatment is offered

separately. While descriptively satisfactory, this detailed break-down is, by itself, unwieldy, difficult to use in discussion and for ready comparison, and not well adapted to the historical nature of the problems. Personally, I would prefer types, or sub-stylistic divisions of a conceptual order (as the style is conceptual), rather than a perceptual dividing of the style. This would undoubtedly reduce the number of "types," as well as facilitate the reader's comprehension of the classificatory units. Although Bennett states that no chronological separation of any of the styles dealt with is possible on the basis of his results, there are certainly strong hints of potential separations which may have a time and space significance. The contents of the Classic Recuay complex of the subterranean galleries as opposed to the other Recuay pottery group in the galleries at Shankaiyan suggests a separation within the Recuay style which it might be well to recognize by conceiving of the two variants as types.

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AFRICA AND ASIA

Tribal Legislation among the Tswana of the Bechuanaland Protectorate: A Study in the Mechanism of Cultural Change. I. SCHAPERA. (Monographs on Social Anthropology, No. 9, 101 pp. 9s. London School of Economics and Political Science, London. 1943.)

Legislation is an intrument of social planning, a technique for explicit formulation of laws. Although, indeed, not all legislation is by any means the consequence of carefully considered thought, all legislation is purposeful. And that purpose is to peg down, if possible, some desired social action. Its immediate purpose may be to crystallize some new form of action desired by the law makers, or to suppress new tendencies not desired by the law makers, or to lay to rest with a legal headstone old practices that have become obsolescent but not yet quite dead.

Not all law making is through means of legislation, either in primitive society or civilized. It is, as a matter of fact, quite evident that among primitives more law emerges through the action of the adjudicative process than through legislative fiat. Yet legislation in primitive society is apparently of much greater significance than we have in the past been willing to recognize.

In this study Professor Schapera is not primarily concerned with "primitive" legislation. For this monograph is, as the subtitle indicates, the statement of an investigation into one aspect of the acculturation processes. Nevertheless Schapera clearly demonstrates that legislative power in the hands of the chief, counselled by his counselors, is ancient tribal practice among the Tswana Bantus. The procedure in law making reveals a device of administrative democracy which in general belies the superficial appearance of autocratic determination of law by the ruling chief. The chief promulgates the law. But ordinarily he does not act without first sounding his senior counselors, his "uncles." If their approval is forthcoming, he calls in the larger council of ward and village head men, finally to submit the matter to the general council of all adult men.

However, it does occur in modern cases that chiefs not infrequently enunciate

laws without popular consent and by arbitrary fiat. And in Bechuanaland, as elsewhere, such laws are popularly sabotaged until withdrawn or allowed to sink into desuetude.

Schapera's materials clearly show that the area within which legislation can control or even influence social change is limited. Nevertheless, within this area legislation is and can be of vital significance. The author attempts to define the boundaries of this area of leeway; but he does not quite succeed in drawing them into sharp clarity. We should not complain if what the author has accomplished is not perfection, but we do need so much more light on this particular problem than is at hand. We as yet know so little about what types of behavior can be legislated into existence or into oblivion. It is up to anthropology to find what cultural dynamics has to offer on this vital point at a time when the trend in the modern world is to experiment and often blunder with more and more legislative control.

Professor Schapera's data lead him to conclude that the personal element is a large factor in the operation of legislation as an instrument of cultural adjustment for the Tswana. Certain chiefs have attempted almost no law making. Certain others have a large list of laws to their credit. Others have tried law making with indifferent success. Granted that other factors are also in the picture, we cannot but agree with Schapera that the very personality of the chief influences and in some instances determines whether a particular change or set of changes is forced in the culture or not.

Legislative activity has increased since the beginning of the period of the European contact. This is not because of any tendency to imitate the occidental law-making itch, but rather obviously because adjustment to change has been more frequently and drastically called for than has been the case in earlier times.

It is interesting that only a small part of the new laws attempts to introduce new practices adopted from Christianity or European civilization. However, many of them undertake to suppress old practices not compatible with the morality of Christianity, such as bride-price or polygyny. Circumcision comes under this rubric, too, not because circumcision *per se* is unchristian, but because it is a part of a ceremonial and organizational structure which is not compatible with Christianity.

The prohibition of liquor is the most common type of legislative repression imposed by the chiefs. It is with equal frequency the type of legislation which most consistently fails. Other common legislation controls trade with Europeans and seeks to further the conservation of natural resources, an area of action in which the British administration is often the the originating source behind the law. Occasionally, legislation giving the status of law to traditional usage crops up, but it is interesting to note that no great reliance on statutory law as a preserver of old custom is to be found.

This monograph on Tswana legislation is more exploratory than definitive. It is also limited in its scope by reason of the fact that it is intended to serve merely as a supplement to the author's well-known *Handbook of Tswana Law and Custom*. As an acculturation study it serves to carry the old law into the present, but only insofar as the old law has been modified by legislative action on the part of the chief.

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Monuments to the Dead of the Bhils and Other Primitive Tribes in Central India. A Contribution to the Study of the Megalith-Problem. W. KOPPERS. (Annali Lateranensi 6, pp. 117–206. Citta del Vaticano, 1942.)

This study, partial result of fourteen months' field work among some of the aboriginal peoples of Central India, is a preliminary attempt on the basis of available information to define in terms of racial and linguistic group the megalithic culture of India, and to offer suggestions regarding its origin and its relations with megalithic cultures in other parts of the world. A survey of monuments to the dead and attendant ceremonial practices among the Bhils leads Koppers to conclude that the carved stone slabs erected as memorials to the dead owe their origin to the neighboring Rajputs and Gujars, and that in all likelihood the monuments do not represent development from an earlier and more simple form indigenous to the Bhils. On the other hand, the Korkus and Gonds in the same general area and, to the east, the Mundas, display a more highly developed megalithic complex in connection with the cult of the dead.

From these facts Koppers draws several tentative conclusions. Since the Bhils are in all probability non-megalithic, it follows that they should be classed with neither the Dravidian nor the Munda groups, and that together with certain other Central Indian peoples, among whom are the Baiga, the Bhils probably should be considered as pre-Dravidian and pre-Munda. Other evidence bearing on this point has been discussed by Koppers in previous articles.[1] It is worth noting that Koppers' views on this subject are at variance with those of other investigators. R. V. Russell[2] is inclined to consider that the Bhils should be classed with the Munda-speaking peoples, and Verrier Elwin,[3] following Russell, regards it as probable that the Baigas also belong to this group. In the present state of our knowledge, however, definite conclusions are premature; Koppers' hypothesis may well prove a useful line of inquiry in further research.

Koppers does not attempt to give a positive answer to the question: what racial group is to be regarded as the original exponent of megalithic culture in India. The tentative suggestion is put forth that both Dravidian Gonds and "Austroasiatic" Mundas were influenced in this respect by a third group which may have been representatives of the "Austronesian" peoples. "As far as language is concerned, the Austroasiatics at least are certainly closely connected with these Austronesians" (p. 200).

Koppers finds it significant that both in Europe and Central India megalithic monuments are connected with the cult of the dead, and that in both cases the "cultural stage of development" is analogous. Therefore, "we may surely venture to assert that there is more to be said for than against the possibility of a genetic, historical connection between the two, though this connection need not of course have been in any sense a direct one" (p. 204).

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[1] *Anthropos*, vol. 35–36, 1940–41, and *Internationales Archiv für Ethnographie*, vol. 41, 1942.

[2] *Tribes and Castes of the Central Provinces of India*, vol. 2, p. 279. London, 1916.

[3] *The Baiga*, p. 4. London, 1939.

The Agaria. VERRIER ELWIN. (xxxv, 292 pp., 36 pls., 44 figs., 5 maps, bibliography. Oxford University Press, India, 1942.)

Mr. Elwin is without doubt our foremost authority on the so-called aboriginal tribes of the Central Provinces of India. Hence his information is always sound, detailed and reliable. On the other hand his own familiarity with all matters pertaining to these tribes makes him assume a similar familiarity in others, which explains perhaps the lack of a badly needed glossary, and the omission of all kinds of minor, but needed, explanations throughout the book.

The Agaria are the iron-miners, smelters and smiths of the Central Provinces. Those among the Agaria who take to cultivation experience acute guilt-feelings because they have deserted their foreordained and not too remunerative craft. The term Agaria, used by Elwin as a general designation, covers numerous groups of iron-workers throughout this area. Their possible descent from the pre-Aryan Asura is carefully examined and the probability of this descent is—quite rightly—shown to be rather great. On the other hand customs, etc. vary so much from named sub-group to named sub-group that in the end one wonders whether the term Agaria should not be used simply to designate all non-Hindu iron-workers of that area. The situation is certainly confused, and Mr. Elwin's chapter on social organization is of no great help in clarifying the problem even for the careful reader.

The lives of the Agaria, as well as Mr. Elwin's book, revolve around the cultural theme of iron, whose role in social organization, myth, magic, economics, etc., is carefully examined. The chapter on craft is most meticulous, and almost microscopically accurate in detail, and extremely satisfying even to an anthropologist not primarily interested in material culture.

Unfortunately Mr. Elwin's book is rather poorly written. Ethnographic reports must be organized in one of two ways. One way of organizing data is exemplified by the canned, streamlined, standardized and neatly packaged monographs of our own University presses, in which detailed tables of content, used as pigeonholes, help the author to present the reader with a businesslike card-index disguised as a monograph. The other way is based upon a systematic conceptual analysis, upon theoretical rigorousness, upon the internal order of the induction and deduction process. This second type of organizing one's material is especially well suited for ethnographic reports which revolve around a central cultural "theme," i.e., iron in the case of the Agaria.

It is regrettable that whereas Mr. Elwin is interested in this type of analysis, he has not adopted it uncompromisingly. He flits back and forth between the card-index pattern, Frazerian comparisons, functionalism, psychiatry, etc., with the result that the book has neither internal nor external order, and is hard to read. The data are there, rich, detailed, authoritative; the insight is there, fresh, often subtle, and broad enough, for example, to revive W. H. R. Rivers' unjustly and stupidly neglected great contributions to psychiatry. Yet the Agaria never quite come to life—an incomprehensible thing in a book by the author of "The Baiga."

Field-work is the life-blood of anthropology, but one can spend too much time in the field. What Mr. Elwin needs most at this juncture is a refresher, a plunge into the Pierian spring of the London School of Economics, or of one of the progressive departments

of anthropology in the United States. We hope that in the interest of science some foundation will stake this most distinguished field-worker and scholar to such a venture.

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A Study of the Leuangiua Language. P. A. LANYON-ORGILL. (24 pp. 2*s*.6*d*. Luzac & Co., London, 1944.)

A Dictionary of the Mailu Language. P. A. LANYON-ORGILL. (Edited and enlarged from the researches of the Rev. W. J. V. Saville and the Comte D'Argigny. 72 pp. 5*s*. Luzac & Co., London, 1944.)

According to Ernst Schwarzenburg's Foreword to the "Dictionary," the author of these slender volumes is a prolific youth, at present serving in the Royal Navy. His purpose is to record, in permanent form, the "more valuable manuscript vocabularies and other unprinted information at present to hand" regarding the languages in the Oceanic area. Dictionaries of Raluana (New Britain), Mandegusu (Eddystone Island), Lamalanga (Raga, New Hebrides), Florida and Vaturanga (Solomon Islands) are scheduled to appear, as well as vocabularies and short studies of the following languages: Yengen and Webias (New Caledonia), Lotora (Maewo, New Hebrides), Mukawa, Roro, Suau and Sinaugoro (British New Guinea), Ugi and Marau Wawa (San Cristoval), Santa Catalina, Petat and Savo (Solomon Islands), and the dialects of Choiseul Island, Epi (New Hebrides) and the Torres Islands. The present booklets, presumably, are typical of the forthcoming series.

Leuangiua is a Polynesian language, here recorded by means of IPA symbols "by way of an experiment," as the author innocently puts it. The seven vowels and nine consonants, as well as length, appear to be phonemic, although long vowels should probably be written double. The symbol '-' is not explained, but appears to indicate morpheme juncture. The morphology and syntax occupy only four pages of the text, and are full of negative statements ("there is no gender, there are no special forms for the third person, there is no conjugation, there are no pure adjectives"), which point to an Indo-European bias. This is not surprising either, since the plan of description (suggested by Radcliffe-Brown) follows Jespersen's "Philosophy of Grammar." The vocabulary, arranged in order of the phonetic alphabet rather than alphabetically (which makes it very hard to use without constant cross reference to the relatively arbitrary order in which the sound-types appear), is followed by a historically speaking, valuable sketch of the linguistic position of Leuangiua among the Polynesian languages.

Mailu is spoken on the south coast of the Eastern Division of Papua, and may be presumed to be a Papuan, not a Melanesian, language. The author quotes his own work, unknown to the reviewer, entitled, "The Languages of the World," to the following effect: "The Papuan languages constitute the most advanced branch of the Australian family, itself the most undeveloped of all linguistical groups." Jespersen wrote, in 1922, that "that language ranks highest which goes farthest in the art of accomplishing much with little means, or, in other words which is able to express the greatest amount of meaning with the simplest mechanism." One wonders, even while willingly suspend-

ing one's disbelief in the evolutionary illusion, what mechanism, that is, structure, is available for comparative examination, a necessary basis for any historical statement. The grammatical notes tell little more than that "there is no distinction of case in the Indo-European sense of the term," "adjectives . . . are not compared" (what is an "adjective"?), "pronouns have no gender," etc. Other Papuan languages, and the languages of Australia, also need structural description before the story of their development can be told. The vocabulary, here alphabetically arranged, is based on Saville's unpublished dictionary, Ray's published vocabularies, the Comte d'Argigny collection of words, and some other sources. The compilation, again, is of historical value.

THOMAS A. SEBEOK

INDIANA UNIVERSITY

The Golden Wing, a Family Chronicle. LIN YUEH-HWA. (Foreword by Bruno Lasker. iii, 175 mimeographed pp. $2.00. International Secretariat, Institute of Pacific Relations, New York, 1944.)

China presents an amazingly rich treasure-house for the anthropologist or sociologist. In almost any one of its small villages, even today, may be found, side by side with the utmost refinements and sophistications of a mature and highly complex civilization, "primitive" traits and institutions such as may be traced to centuries or even millennia ago. It is cause for genuine wonderment, therefore, that so little study of truly scientific value has been made on Chinese society, despite the abundance of popular generalizations on the subject.

Chinese scholars themselves, educated in the past along strictly classical and humanistic lines, have devoted their major efforts to historical, philosophical and literary studies. Western sinologists have for the most part followed suit; anthropologists, confronted by the difficulties of the Chinese language and literature, have turned their attention to more "primitive" societies; and the western businessman or diplomat in China has usually been too busily engaged in making money or delving into Chinese politics to find time for more cultural pursuits. Thus the describing and interpreting of Chinese society have been left, largely, in the hands of missionaries: men whose lack of scientific training and frequently prejudicial conceptions have often ill equipped them for the task. As a consequence, scholars have long been forced, for want of anything better, to turn to such works as Arthur H. Smith's *Chinese Characteristics* (London & N. Y., 1892) and *Village Life in China* (London & N. Y., 1899), despite the prejudices and inaccuracies found in these books.

The last ten or fifteen years, fortunately, have seen a great change for the better in this respect. A growing number of young Chinese scholars, trained in the scientific techniques of the West, yet with firm roots in their own agrarian, non-industrialized society, have come to realize that the study of Chinese civilization is to be found not only in books, but also in the lives of the contemporary persons around them. This realization has led to such works as Chen Han-seng's *Landlord and Peasant in China* (N. Y., 1936); *Agrarian China, Selected Source Materials from Chinese Authors*, with introduction by R. H. Tawney (Chicago, 1938); Fei Hsiao-tung's *Peasant Life in China* (N. Y. & London, 1939); and H. Y. Lowe's *The Adventures of Wu: The Life Cycle of a*

Peking Man (Peking, 2 vols., 1940 and 1941).[1] *The Golden Wing* is the latest valuable addition to this series.

Dr. Lin, author of *The Golden Wing*, received his training at Harvard and is now a professor of anthropology at Yenching University (formerly in Peiping, but since 1941 removed to Chengtu, in free China). The locale of his book is the river port of Hookow (Wade-Giles romanization: Ku-k'ou), two miles from his own village birthplace, and eighty miles up the Min River from the great seaport of Foochow, capital of the southeast coastal province of Fukien. Though the book is written ostensibly in fictional form, it is obviously based upon the author's own close observation and knowledge.

The story centers around the careers of two men, Hwang Dunglin and Chang Fenchow, and their respective large clans. Dunglin, its chief hero, is first shown as a young farmer (born ca. 1870), struggling against poverty and hardship. His acquisition of a partnership with Fenchow in a small shop in Hookow marks the rise to fame and fortune of both men, together with their clans. In the case of the Hwang clan, this is symbolized by the building of a magnificent mansion, known as "The Golden Wing," from which the book gains its name. Various difficulties and disasters overtake the two clans, however, eventually causing Fenchow to give up his partnership and die in misery, leaving his clan, by the 1930's, to become dispersed and impoverished. The clan of Hwang, on the other hand, survives misfortune. Yet Dunglin, too, now an old man over seventy, is forced by the Japanese occupation of Foochow in the spring of 1941 to give up his business and return to the soil which gave him birth. The book ends as the flight of a Japanese airplane causes his grandsons to look up anxiously from their work in the field. The old man turns to them quietly with the admonition: "Sons, you forget to put your seeds into the earth."

In the course of this narrative, presented with a minimum of personal comment or interpretation, we are given objective descriptions of the round of toil on a Chinese farm; the intricate pattern of economy linking Foochow with the up-river port of Hookow, and penetrating from there into the hinterland; the annual customs and festivals, and birth, marriage and funeral rites of South China; bandit incursions and civil war; political corruption and the operations of the law courts; clashes between the traditional way of life and new ideas and technologies introduced from the West; and innumerable other interesting topics. Some readers may perhaps feel that too much attention is sometimes paid to trivial matters. Such is not the complaint of this reviewer, however, who would nevertheless have preferred to read more about the agricultural processes on which some eighty per cent of all Chinese are still directly dependent, and less about the operations of the shop in Hookow.

The fictional form of the book has both merits and demerits. Its greatest merit is the opportunity to show in graphic terms the ups and downs of fortune in a Chinese family and the radical changes that have overtaken Chinese life in recent decades. On

[1] The reviewer would like to call particular attention to this last work, both because it is almost totally unknown in the West, and because, though written in a popular (and sometimes rather quaint) English style by a Chinese who makes no scholarly pretences, it nevertheless presents a remarkably accurate, as well as sympathetic, picture of a well-to-do traditional Peking family. It is furthermore enhanced by an abundance of the author's own line drawings.

the other hand, Dr. Lin is an anthropologist rather than a novelist; his fictionalized treatment is therefore, from an artistic point of view, not wholly successful. His style is matter-of-fact and prosaic, the characters fail to come to life, and his abrupt juxtaposition of widely unrelated incidents (done in order to crowd as much as possible onto his canvas) is mechanical and artificial. Furthermore, his book, in the clarity of its presentation and organization of its subject matter, compares unfavorably with the static, but systematic and topical treatment, followed in such a book as Fei Hsiao-tung's *Peasant life in China*. It lacks, too, the statistical material found in many other field studies.

There are, furthermore, other defects of an editorial nature, for which the author, however, far removed in China, is only in small part responsible. The innumerable typewriting errors that dot virtually each of the book's mimeographed pages should, despite wartime conditions, be a source of regret to such an organization as the Institute of Pacific Relations. The absence of an index, too, imposes an enormous handicap upon the ready consultation of a book organized on a narrative, rather than an analytical, framework. For example, though chapter 6 is entitled "The Village Festivities," yet one of the most important of such celebrations, that of the Dragon Boat, occurs in Chapter 5, entitled "The Primary School," while two others (those of Filial Piety and Pure Brightness) are found in Chapter 8. Even if conditions made the compilation of an index unfeasible, the use of detailed sub-titles under chapter headings would have done much to remedy this lack. And finally, it is regrettable that the author has not provided a greater number of Chinese technical terms for the things he describes, coupling them, if possible, with Chinese characters, but in any case rendering them in the standard Wade-Giles system of romanization, rather than in an apparently haphazard romanization of his own.

In spite of these defects, however, *The Golden Wing* is a valuable addition to our knowledge of Chinese society. It is detailed, comprehensive and accurate;[2] its narrative form enables it to show vividly the changes that have taken place in rural China in the last few decades; and it describes an important area (Fukien province) heretofore inadequately treated save for J. J. M. de Groot's monumental *Fêtes annuellement célébrées à Émoui (Amoy)* (Paris, 1886)—a work which (as indicated by its title) is much narrower in scope.

DERK BODDE

UNIVERSITY OF PENNSYLVANIA

MISCELLANEOUS

The Science of Man in the World Crisis. RALPH LINTON, Editor. (532 pp. $4.00. Columbia University Press, New York, 1945.)

In the introductory chapter of this ambitious volume Dr. Linton presents the view that the time is ripe for a new synthesis of the sciences dealing with man and his problems. He nominates anthropology for the task because of its willingness to employ the

[2] The only inaccuracy noticed by the reviewer is the statement on p. 110 that the twenty-four annual festivals or "joints" in the Chinese year are of lunar origin. While it is true that much of the Chinese calendar is lunar, these "joints" (including as they do the solstices and equinoxes) are obviously based on solar reckoning.

techniques and conclusions of other disciplines. Most sciences have concentrated on phenomena of certain limited sorts, and even anthropology has had periods when it seemed destined to develop into separate and very special fields. Fortunately the tendency to closer integration is now evidenced by such developments as the interest of physical anthropologists in studies of a more dynamic sort with more attention paid to cultural factors. Certain new trends, associations, and interests are also evident. The long-time contact between ethnology and sociology has been to the advantage of both; the use of anthropological data in colonial administration and industrial relations has given it standing in the field of applied science; now a new field of concentration is emerging—one which deals with the interrelations between personality and culture.

To bring out the common problems of these various interests Dr. Linton has enlisted the aid of twenty-one anthropologists, sociologists, psychologists and others. The result is a volume of wide range but much better integrated than most collaborative studies.

Two recognized fields of anthropology are not represented. The ultimate value of linguistics in an understanding of human behavior is accepted, but the editor finds its offerings at present too meagre for inclusion. Archaeology is also ruled out of the current discussion although its conclusions with regard to process are recognized as fundamental to all interested in the phenomena of cultural change.

Limits of space make detailed comments on the many articles impossible but brief references to a few will give the reader some idea of the scope and value of the volume. The papers by Shapiro, Krogman and Kleinberg deal with race, race differences and their relation to social status and group attitudes, while Wirth throws emphasis on the problems of minority groups. Two challenging chapters by Kennedy and Keesing should be put on the *must* list for all who are interested in colonial problems and future world peace.

Among other significant chapters are those by Kluckhohn and Kelly dealing with the concept of culture, the scholarly discussions of cultural change and aspects of acculturation by Herskovits and Hallowell, the psychological appróach of Kardiner and Dollard.

The range of interests and number of writers bar even brief mention of each. Some fail to fit into the integrated study outlined in the Introduction, but the overall verdict is that the "new synthesis" is well under way.

FAY-COOPER COLE

UNIVERSITY OF CHICAGO

The Cultural Background of Personality. RALPH LINTON. (xix, 157 pp. $1.50. D. Appleton-Century Co., New York, 1945.)

One of the most fruitful collaborations of our time has been that between the anthropologist, Linton, and the psychiatrist, Kardiner, and the people who participated with them in their joint seminar at Columbia. Certainly the results stemming from this collaboration are more than an ordinary milestone in the development of social anthropology. Beside a rich and varied field experience Linton brought to the joint effort a happy ability to see fundamental problems and to stake them out with well-defined concepts. This capacity had already been amply demonstrated in his "Study of Man" and in published articles such as those on age and sex categories.

In the present work Linton attempts to make intelligible order out of the field of interest for which we as yet have no suitable term, but which we lamely refer to, more often than not, as "culture and personality." This reviewer, for one, hopes no one will hit upon a good term—a term which would thereby isolate a body of knowledge that must some day be the keystone of all social science, a keystone that will merge imperceptibly with the whole design of the arch.

The intelligible order which Linton attempts is taken from a collection of facts and insights that have been gathering around the interest in the problems of how personality is formed and what is the actual content of personality. He does this by formulating a series of very useful concepts. But first he lays out the general context of personality and culture within which these concepts become useful and operative. The little book is such a tightly integrated progression, fertile with insights, that it is impossible to summarize it. It is a precise statement and formulation of problems toward which many of us in recent years have been working.

Linton, however, is perfectly aware that we are still in a frontier stage, as these two quotations show:

"Pending the development of new techniques suited to the particular qualities of personality, culture, and society, investigators must arrive at their conclusions through the simple observation and comparison of their materials. Such an approach is comparable to that of the old-style naturalists' rather than that of the modern student of animal behavior. However, it must not be forgotten that without the orientations provided by the naturalists' work many of the later developments would have been impossible." (p. 4)

"In bringing this discussion to a close I am keenly conscious of the number of problems which I have indicated without being able to provide solutions. I am also conscious of the extent to which I have had to depend on techniques which will appear unscientific to those who regard science as something inseparably linked with the laboratory and slide rule. Those who are investigating culture, society and the individual and the complex interrelations of these phenomena are pioneers and, like all pioneers, they have to live by rough and ready methods. They are laboring on the lonely outposts which science has set up on the fringes of a new continent. . . . The pioneers can only press on, sustained by the belief that somewhere in this vast territory there lies hidden the knowledge which will arm man for his greatest victory, the conquest of himself." (p. 152)

Linton's "The Cultural Background of Personality" will be found helpful as an opening book in any course that deals with the individual and his culture. The reviewer used it in such a course at the time it came out—with good effect on students and instructor.

SCUDDER MEKEEL

UNIVERSITY OF WISCONSIN

The Dynamics of Culture Change. BRONISLAW MALINOWSKI. (Edited by Phyllis M. Kaberry. xiv, 171 pp., tables. $2.50. Yale University Press, New Haven, 1945.)

Whether or not one agrees with all the theories and statements set forth in this posthumous book, the fact that such a well-organized work could be produced from mere scattered notes, previous publications and lectures attests to the fundamental consistency and vigor of Malinowski's thought and principles, as well as to the unusual editorial ability of Dr. Kaberry. The book is divided into two main sections, Part I

being a discussion of various methods employed in studying culture contact and change, and Part II the application of the author's theory to specific institutions. For the illustration of his theories and methods the author has drawn his material primarily from South and East Africa.

The first chapter deals with the "new tasks of modern anthropology," by which is meant the study of culture change which "must become one of his [the anthropologist's] main tasks in field work and theory," for the simple reason that "the uncontaminated Native does not exist, anywhere." The author admits the difficulty of handling the subject matter of culture change both theoretically and methodologically, and concludes that changing Africa is not a single unit but one composed of three phases: (1) the Western influences at work, (2) Native African attitudes and customs and, (3) those present institutions which are the result of contact and change, i.e., "the order of transition." In Chapters II, III, and IV various theories are discussed, one of the most striking features of which is Malinowski's more liberal attitude concerning the value of history. He feels however that a distinction must be made between history, or the past, which is dead and buried and "those elements of the old culture which still retain their vitality and influence present conditions." Instead of projecting these elements in a time-scheme of past, present, and future they are to be listed under categories all of which co-exist in the present. For this Malinowski proposes a three-column chart (which may be developed into a four, five or even six-column chart) in which the various items are placed each in its own column against the corresponding evidence in others. In column A are entered White influences, interests and intentions; in column B the processes of culture contact and change; in column C the surviving forms of tradition. Where evidence is available Column D may be added to include the reconstructed past, and a column E to take care of the new forces of spontaneous African re-integration or reaction, of which African nationalism may be cited as an example. In holding this view of history Malinowski again stresses his opposition to Professor Herskovits' position which, Dr. Kaberry states in the Introduction, "constitutes something of a challenge to the premises of this book." It is interesting to note however, that by the inclusion of column D, i.e., the reconstructed past, Malinowski makes a concession to the value of Professor Herskovits' theory regarding the importance of the reconstructed past in acculturation studies. Desirable as a rapprochement between the historical and functional schools certainly is, the step which seems to be taken here is not entirely satisfactory since column D hardly follows logically the arguments presented for the functional method.

The second part of the book applies practically to specific institutions the method developed in the first part. The several chapters are concerned with African warfare, African witchcraft and other social phenomena including the, for Africa, important question of indirect rule. The chart showing the factors at work in the belief in witchcraft draws a very nice overall picture of the situation. The sincere desire of the Whites to stamp out witchcraft (col. A) has its perfect parallel in the Native's wholehearted desire to be rid of it (col. C), and therefore the common factor (col. B) is the apparent ideal of sympathetic collaboration between Native informants and the enlightened enquirer. This leads to (col. A) the summary proscription of belief in magic while col. C shows that a firm belief still survives in magic, both good and bad, and results

in being driven underground (col. B) where it flourishes under conditions of change in towns and locations as much as on reserves. Therefore, there is complete bewilderment and the impression that European law abets sorcery, which arouses resentment. Ignorance on the part of the Whites of the real psychological and social mechanisms (col. A) is again paralleled by the Native beliefs (col. C) based on powerful foundations, and thus the Natives register an increase in witchcraft and sorcery and blame the European missionary and administration for it, while the real cause is the increase in misery, ill health, and the impossibility of controlling poverty paralleled by increasing rivalries among the Natives (col. B). As can be seen from this short example the charts may be an aid in clarifying factors involved in culture change, but in the opinion of the reviewer their importance has been over-emphasized in this book. Such a technique while attempting to give a clear and objective picture may, by formalizing descriptive categories, result in encouraging rather than in eliminating subjectivity.

For the specialist the book as a whole offers little or nothing which is absolutely new since it presents topics and theories with which Malinowski has already dealt in previous publications and lectures, but it does serve to clarify some of his earlier statements and viewpoints and to bring them together in a pointed and concise form. Furthermore, it provides not only interesting but stimulating reading, as Malinowski always does.

H. Engel Hause

University Museum
University of Pennsylvania

A Scientific Theory of Culture and Other Essays. Bronislaw Malinowski. (Preface by Huntington Cairns. ix, 228 pp. The University of North Carolina Press, Chapel Hill, 1944.)

This posthumous volume comprises three essays, A Scientific Theory of Culture (pp. 1–144), The Functional Theory (pp. 145–176), and Sir James George Frazer (pp. 177–221). Of these, the last-mentioned with its generous appreciation of a distinguished scholar is the most appealing, irrespective of the merits of the several value judgments. Indeed, sporadically the book shows the author in his most benign mood, as when he recognizes the antiquity of "functionalism" and deprecates having done more than labeling "an existing body of doctrine, method, and interest" (p. 147) Elsewhere, however, he is conspicuously unfair. He stigmatizes Graebner, who doubtless is open to legitimate criticism enough, quite undeservedly for having created "an antifunctional approach of first-rate imbecility" (p. 149), and excoriates the surely extinct, if not wholly mythical, field worker who has "his eye exclusively on the exotic or picturesque" (*ibid.*). Elsewhere there is no explicit acknowledgment of others' priority. Thus, I fail to see that the assault on "survivals" (p. 28 ff.) says a whit more than what Marett said before 1918, several years before Malinowski impinged on the Anglo-Saxon scene (reprinted essays in R. R. Marett, "Psychology and Folk-Lore," 26, 101 f., 1920).

However, one does not read so temperamental a writer as Malinowski for a balanced historico-critical account of anthropological views. The book as a whole claims at-

tention rather as the definitive professional credo of one of our most colorful personalities.

Since Malinowski (p. 38) joins the chorus of those who periodically rave against the view of culture as "a thing of shreds and patches" (thus wrote Gilbert; not as Malinowski has it, "patchwork of shreds and tatters"), I may be permitted to explain that my quotation from "The Mikado" in the last paragraph of "Primitive Society" bears a meaning utterly different from that generally ascribed to it. That paragraph should be connected with (a) the quotation from Maitland several pages previously, and (b) with the world situation after 1918. It was primarily a criticism of *our* civilization, not an anthropological theory of culture in general that the contumelious phrase was designed to convey. Beyond that I intended to spurn the widespread notion that "progress" is necessarily "organic growth," resting on "historical continuity." As Maitland indicates, the Anglo-Saxons "did not arrive at the alphabet or at the Nicene Creed by traversing a long series of 'stages'; they leapt to the one and to the other." The possibility and frequency of such mutations I still maintain and hope ere long to demonstrate on a larger scale.

ROBERT H. LOWIE

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SOME NEW PUBLICATIONS

ABBREVIATIONS

AA	American Antiquity. Menasha.
AcAm	Acta Americana. Los Angeles.
AI	América Indígena. Mexico, D.F.
AJPA	American Journal of Physical Anthropology. Philadelphia.
AmS	American Scientist. Champaign, Ill.
AR	Anthropological Records. Berkeley and Los Angeles.
AS	African Studies. London.
ASGH	Anales de la Sociedad de Geografía e Historia. Guatemala.
BAEB	Bureau of American Ethnology Bulletin. Washington.
BBMB	Bernice P. Bishop Museum Bulletin. Honolulu.
BBMSP	Bernice P. Bishop Museum Special Publications. Honolulu.
BI	Boletín Indigenista. Mexico, D.F.
BMFEA	Bulletin of the Museum of Far Eastern Antiquities. Stockholm.
BMN	Boletim do Museu Nacional. Rio de Janeiro.
CINMAAE	Carnegie Institute Notes on Middle American Archaeology and Ethnology. Washington.
CP	Character and Personality. Durham, N. C.
ES	Etnologiska Studier. Göteborg.
GR	Geographical Review. New York.
IJAL	International Journal of American Linguistics. Baltimore.
JAFL	Journal of American Folklore. Menasha.
JAOS	Journal of the American Oriental Society. Baltimore.
JASP	Journal of Abnormal and Social Psychology. Albany, N.Y.
JPS	Journal of the Polynesian Society. Wellington, N.Z.
JRAI	Journal of the Royal Anthropological Institute. London.
JRAS	Journal of the Royal Asiatic Society. London.
JSP	Journal of Social Psychology. Worcester, Mass.
MA	The Minnesota Archaeologist. Minneapolis.
MASB	Massachusetts Archaeological Society Bulletin. Andover.
NMLP	Notas del Museo de la Plata. La Plata.
PA	Pennsylvania Archaeologist. Harrisburg.
PDS	Philological Documentary Studies. New Orleans.
PM	Primitive Man. Washington.
PPMAAE	Papers of the Peabody Museum of American Archaeology and Ethnology. Cambridge.
RI	Revista de Indias. Madrid.
RLIJ	Rhodes-Livingstone Institute Journal. Livingstone, Northern Rhodesia.
RMEA	Revista Mexicana de Estudios Antropológicos. Mexico.
RMLP	Revista del Museo de La Plata. La Plata.
RSAA	Relaciones de la Sociedad Argentina de Antropología. Buenos Aires.
SA	Sovetskaia Arkheologiia. N. I. A. Marr Institute for the History of Material Culture, Academy of Sciences of the USSR. Moscow.
SAJS	South African Journal of Science. Johannesburg.
SE	Sovetskaia Etnografia. Ethnographic Institute of the Academy of Sciences of the USSR. Moscow.
SJA	Southwestern Journal of Anthropology. Albuquerque.
SM	Scientific Monthly. Washington.
TTT	Tlatelolco a través de los tiempos. Mexico.

UCPA:AS University of Chicago Publications in Anthropology. Archaeological Series. Chicago.
UCPAAE University of California Publications in American Archaeology and Ethnology. Berkeley and Los Angeles.
YAS Yale Anthropological Studies. New Haven.
YMT Yikal Maya Than. Yucatan.

North America

Barton, R. *Quelques nouvelles recherches sur le folklore américain* (In Russian) (SE, 1, pp. 224–229. 1938).
Black, Glenn A. *Supplementary Information on "T D" Pipes* (MA, 11, No. 2, p. 50. 1945).
Burgesse, J. Allan. *The Spirit Wigwam as Described by Tommie Moar, Pointe Bleue* (PM, 17, Nos. 3 and 4, pp. 50–53. 1944).
Property Concepts of the Lac-St-Jean Montagnais (PM, 18, Nos. 1 and 2, pp. 1–25. 1945).
Carter, George F., and Edgar Anderson. *A Preliminary Survey of Maize in the Southwestern United States* (Annals of the Missouri Botanical Garden, 32, pp. 297–322. 1945).
Collier, Donald. *Conjuring among the Kiowa* (PM, 17, Nos. 3 and 4, pp. 49–53. 1944).
Cooper, John M. *The Shaking Tent Rite among Plains and Forest Algonquians* (*Ibid.*, pp. 60–84. 1944).
Davidson, Levette J. *Mormon Songs* (JAFL, 58, No. 230, pp. 273–300. 1945).
Densmore, Frances. *Prelude to the Study of Indian Music in Minnesota* (MA, 11, No. 2, pp. 27–31. 1945).
Fenton, William N. *Place Names and Related Activities of the Cornplanter Senecas* III. *Burnt-House at Cornplanter Grant* (continued) (PA, 15, No. 3, pp. 88–96. 1945).
Flannery, Regina. *The Gros Ventre Shaking Tent* (PM, 17, Nos. 3 and 4, pp. 54–59. 1944).
Flaskerd, George A. *Keel Type Stone Pipe* (MA, 11, No. 2, p. 49. 1945).
Copper Axe. Willor River, Aitkin County, Minnesota (*Ibid.*, p. 48).
Some Folsom and Yuma Type Points from Minnesota (*Ibid.*, pp. 32–33).
Geary, James A. *The Changed Conjunct Verb (Without NI) in Fox* (IJAL, 11, No. 3, pp. 169–181. 1945).
Griffin, James B. *The Box Elder Mound in La Salle County, Illinois* (AA, 11, No. 1, pp. 47–48. 1945).
Harrington, M. R. *Farewell to Borax Lake* (The Masterkey, 19, No. 6, pp. 181–184. Los Angeles, 1945).
Harris, Jesse W. *The Catskin Legend in Southern Illinois* (JAFL, 58, No. 230, pp. 301–305. 1945).
Heizer, Robert F. *Honey-Dew "Sugar" in Western North America* (The Masterkey, 19, No. 5, pp. 140–145. Los Angeles, 1945).
Hill, W. W. and Dorothy W. Hill. *Navaho Coyote Tales and Their Position in the Southern Athabaskan Group* (JAFL, 58, No. 230, pp. 317–343. 1945).
Humphrey, Norman Daymond. *The Stereotype and the Social Types of Mexican-American Youths* (JSP, 22, pp. 69–78. 1945).
Jones, Volney H. *The Use of Honey-Dew as Food by Indians* (The Masterkey, 19, No. 5, pp. 145–149. Los Angeles, 1945).
Kluckhohn, Clyde. *A Navaho Personal Document with a Brief Paretian Analysis* (SJA, 1, No. 2, pp. 260–283. 1945).
Kock, Gösta. *Is "Der Heilbringer" a God or Not?* (Ethnos, 8, Nos. 1 and 2, pp. 61–77. Stockholm, 1943).
Landon, R. H., and G. A. Flaskerd. *A Minnesota Point with Folsom and Yuma Characteristics* (MA, 11, No. 2, p. 474. 1945).
The Long Lake Site (*Ibid.*, pp. 34–46).

Leonard, Autumn L. *The Presque Isle Portage and the Venango Trail IV. The French Road* (PA, 15, No. 3, pp. 75–87. 1945).

Magrath, Willis H. *The North Benton Mound: A Hopewell Site in Ohio* (AA, 11, No. 1, pp. 40–46. 1945).

Malouf, Carling. *Considerations in the Counselling of Indians* (Separaide, No. 12, Sept. 7. 1945).

Mook, Maurice A. *Algonkian Ethnohistory of the Carolina Sound* (Journal of the Washington Academy of Sciences, 34, Nos. 6 and 7, pp. 181–228. 1944).

Moore, Allen L. *A Minnesota Catlinite Pipe* (MA, 11, No. 2, p. 26. 1945).

Radin, Paul. *The Road of Life and Death* (xiv, 345 pp. $4.50. Bolling Series, Pantheon Books, Inc., New York. 1945).

Reichard, Gladys A. *Linguistic Diversity among the Navaho Indians* (IJAL, 11, No. 3, pp. 156–168. 1945).

Distinctive Features of Navaho Religion (SJA, 1, No. 2, pp. 199–220. 1945).

Ritchie, William A. *An Early Site in Cayuga County, New York* (Researches and Transactions of the New York State Archaeological Association, 10, No. 1. 92 pp., 58 pls., map. Rochester, 1945).

Rogers, Malcolm J. *An Outline of Yuman Prehistory* (SJA, 1, No. 2, pp. 167–198. 1945).

Smith, William Neil. *The Papago Game of "Gince Goot"* (The Masterkey, 19, No. 6, pp. 194–197. Los Angeles, 1945).

Speck, Frank G. and Ernest S. Dodge. *Amphibian and Reptile Lore of the Six Nations Cayuga* (JAFL, 58, No. 230, pp. 306–309. 1945).

Stuart, Bradley R. *Pug-A-Roo Picks Pine-Nuts* (The Masterkey, 19, No. 5, p. 155. Los Angeles, 1945).

Vinnikov, I. *Un manuscrit inédit de L. Morgan sur les danses des indiens* (In Russian) (SE, 1, pp. 167–184. 1938).

Walker, Edwin F. *The Dig at Big Tujunga Wash* (The Masterkey, 19, No. 6, pp. 188–193. Los Angeles, 1945).

Waring, Antonio J. *The De Luna Expedition and Southeastern Ceremonial* (AA, 11, No. 1, pp. 57–58. 1945).

Watkins, Frances F. *Hopi Game of Shinny* (The Masterkey, 19, No. 5, pp. 162–164. 1945).

Webb, Wm. S., and Charles E. Snow. *The Adena People* (Publications of the Department of Anthropology and Archaeology, University of Kentucky, 6. 369 pp., 28 figs., 1 map, charts. $3.00. Lexington, 1945).

Wilford, Lloyd A. *Three Village Sites of the Mississippi Pattern in Minnesota* (AA, 11, No. 1, pp. 32–40. 1945).

Middle America

Acosta, Jorge R. *La tercera temporada de exploraciones arqueológicas en Tula, Hgo., 1942* (RMEA, 6, No. 3, pp. 125–164. 1944).

Apenes, Ola. *The Primitive Salt Production of Lake Texcoco, Mexico* (Ethnos, 9, No. 1, pp. 35–40. 1944).

Armillas, Pedro. *Oztuma, Gro., Fortaleza de los Mexicanos en la frontera de Michoacan* (RMEA, 6, No. 3, pp. 165–175. 1944).

Barlow, R. H. *Documentos del Archivo del Colegio Imperial de la Santa Cruz* (TTT, 5, pp. 51–55. 1945).

Beals, Ralph L. *The Contemporary Culture of the Cáhita Indians* (BAEB, 142. 244 pp., 20 pls., 33 text figs., map. $.50. 1945).

Bonilla, Marcelina. *The Indians of Honduras* (AI, 5, No. 3, pp. 245–249. 1945).

Caso, Alfonso. *La fecha de la conquista de Tlatelolco por Tenochtitlan* (TTT, 5, pp. 30–31. 1945).

Charlot, Jean. *El San Cristóbal de Santiago Tlatelolco, palimpsesto plástico* (*Ibid.*, pp. 46–50).

Comas, Juan. *El maestro y el médico en el majoramiento indígena* (AI, 5, No. 4, pp. 318–325. 1945).

Espejo, Antonieta. *Las ofrendas halladas en Tlatelolco* (continuación) (TTT, 5, pp. 15–29, 12 pls. 1945).

Gamio, Manuel. *La producción agrícola y la industrialización de los Ejidatarios* (AI, 5, No. 4, pp. 303–308. 1945).

Goubaud Carrera, Antonio. *"El Durazno," sitio arqueológico en el departamento de Jalapa, Guatemala. Apuntes de viaje* (ASGH, 20, No. 2, pp. 105–108. 1945).

Lombardo Otero, Rosa María. *La mujer Tzeltal* (76 pp., 21 pls. Mexico, 1944).

Lowe, S. K. *Paleographic Guide for Spanish Manuscripts, Fifteenth-Sixteenth Centuries: Roman Numerals* (PDS, 2, No. 1, 12 pp., 5 figs., 1 pl. 1943).

McAfee, Byron, and R. H. Barlow. *Anales de la conquista de Tlatelolco en 1473 y en 1521* (TTT, 5, pp. 32–45, 3 pls. 1945).

Pike, Kenneth L. *Tone Puns in Mixteco* (IJAL, 11, No. 3, pp. 129–139. 1945).

Redfield, Robert. *Ethnic Groups and Nationality* (AI, 5, No. 3, pp. 235–245. 1945).

Salazar Ortegón, Ponciano. *Exploraciones arqueológicas en Santiago Tlatelolco* (TTT, 5, pp. 11–14. 1945).

Schultze-Jena, Leonhard. (Carrera, Antonio Goubaud, y Herbert D. Sapper, translators) *La vida y las creencias de los indios quichés de Guatemala* (ASGH, 20, No. 3, pp. 145–160. 1945).

Spicer, Edward H. *El problema Yaqui* (AI, 5, No. 4, pp. 273–286. 1945).

Termer, Franz. *Ein Besuch bei den Xinca-Indianern in Südostguatemala* (Ethnos, 9, Nos. 3 and 4, pp. 97–117. Stockholm, 1944).

Thompson, J. Eric S. *Escritura jeroglífica. Aritmética y astronomía de los Mayas* (Enciclopedia Yucatanense, 2, pp. 309–342, tables and plates. 1945).

A Survey of the Northern Maya Area (AA, 11, No. 1, pp. 2–24. 1945).

Wassén, Henry. *A Forged Maya Codex on Parchment. A Warning* (ES, Nos. 12 and 13, pp. 293–304, 8 figs. 1941).

Watkins, Frances E. *Pottery of Chiriqui, Panama* (The Masterkey, 19, No. 6, pp. 198–201. Los Angeles, 1945).

South America

Alejo Vignati, Milcíades. *Censo óseo de paquetes funerarios de origen Guaraní* (RMLP, n. s. 2, pp. 1–11. 1941).

Antigüedades en la región de los Lagos Nahuel Huapi y Traful. I–VII (NMLP, 9, No. 23, 165 pp., 58 figs., 28 pls., 8 maps. 1944).

Iconografía aborigen I. Los caciques Sayeweke, Inakayal y Foyel y sus allegados (RMLP, n. s. 2, pp. 13–48, 28 pls. 1942).

Iconografía aborigen II. Casimiro y su hijo Sam Slick (*Ibid.*, pp. 225–236).

Botelho de Magalhaes, Amilcar. *Indios do Brasil* (AI, 5, No. 4, pp. 309–315. 1945).

Belli, L. *La deidad votiva pluvial en la civilización nazca* (ASGH, 20, No. 2, pp. 119–123. 1945).

Bryan, Kirk. *Recent Work on Early Man at the Gruta de Cadonga in the Argentine Republic* (AA; 11, No. 1, pp. 58–59. 1945).

Castellvi, Fray Marcelino de. *Historia eclesiástica de la Amazonia Colombiana* (Universidad Católica Bolivariana, 10, No. 36, pp. 355–374. 1945).

Castro, Josué de. *Areas alimentares do Brasil* (AI, 5, No. 3, pp. 191–205. 1945).

Dickey, Herbert Spencer. *History Repeats* (The Masterkey, 19, No. 6, pp.185–187. Los Angeles, 1945).

García, Antonio. *El indigenismo en Colombia. Genesis y evolución* (AI, 5, No. 3, pp. 217–234. 1945).

Imbelloni, J. *Las investigaciones serológicas del Profesor A. Santiana* (NMLP, 9, Nos. 30 and 31, pp. 439–443. 1944).

Malaret, Augusto. *Los Americanismos a través de los siglos* (Universidad Católica Bolivariana, 11, No. 39, pp. 177–194. 1945).

Márquez Miranda, Fernando. *Los textos millcayac del P. Luis de Valdivia* (RMLP, n. s., 2, pp. 61–223. 1943).

Ott, C. F. *Os elementos culturais da pescaria baiana* (BMN, No. 4, 67 pp., 25 figs. 1944).

Contribuição á arqueologia baiana (BMN, No. 5, 48 pp., 57 figs., 1 map. 1944).

Palacio Atard, Vicente. *El Asiento de la Mina de Huancavelica en 1779* (RI, 5, No. 18, pp. 611–630. 1944).

Reyes, Angel. *The Guambiano Indians* (AI, 5, No. 3, pp. 221–224. 1945).

Rydén, Stig. *Negative Painting among South American Indians—An Apropos* (Ethnos, 8, No. 3, pp. 96–103. 1943).

Notes on the Morê Indians, Rio Guaporé, Bolivia (Ethnos, 7, Nos. 2 and 3, pp. 84–124. Stockholm, 1942).

Santiana, Antonio. *Los grupos sanguíneos de los Indios de Ecuador* (NMLP, 9, Nos. 30 and 31, pp. 431–438. 1944).

White Uribe, Gustavo. *Petroglifos precolombinos* (Universidad Católica Bolivariana, 10, No. 36, pp. 410–422. 1944).

Europe and Asia

Abramson, S. *L'oeuvre du peuple kirghize* (In Russian) (SE, 3, pp. 77–88. 1940).

Aiyappan, A. *Iravas and Culture Change* (Bulletin of the Madras Government Museum, n. s., General Section, 5, No. 1. 204 pp. 12 pls., text figs. 16 rupees, 10 annas. Madras, 1944.

Andersson, J. G. *Some Fossil Mammal Localities in Northern China*. (BMFEA, No. 14, pp. 29–43. 1942.

Researches into the Prehistory of the Chinese (BMFEA, No. 15. 304 pp., 200 pls. Stockholm, 1943).

The Site of Chu Chia Chai, Hsi Ning Hsien, Kansu (BMFEA, No. 17, pp. 1–63. 1945).

Anfimov, N. *Nouvelle contribution à l'histoire du Bosphore asiatique* (In Russian with French résumé) (SA, 7, pp. 258–267. 1941).

Bahder, O. *Le cimetière de Karabaj près du village de Balanova en Tchouvachie* (In Russian with French résumé) (SA, 6, pp. 63–88. 1940).

Barton, R. F. *Analysis of the Oldest Stratum of Ifugao Kinship Terms* (In Russian with English résumé) (SE, 3, pp. 201–206. 1940).

Belov, G. *Contribution à l'étude de la vie économique et sociale des habitants de Chersonèse à la fin de moyen âge* (In Russian with French résumé) (SA, 7, pp. 231–244. 1941).

Berg, Gösta. *Spåntapeter* (Folk-Liv. 7–8, pp. 260–272. Stockholm, 1943–44).

Björkquist, Lennart. *En jämtsk selkroksgrupp* (Folk-Liv, 7–8, pp. 135–142. Stockholm, 1943–44).

Blavatskij, V. *Les bronzes acarnaniens de Lysippe* (In Russian with French résumé) (SA, 7, pp. 189–198. 1941).

Bonet Reverón, B. *Las expediciones á las Canarias en el siglo XIV* (RI, 5, No. 18, pp. 577–610. 1944).

Bukinič, D. *Le costume ancien des Djemchides et des Khazaras de l'URSS* (In Russian) (SE, 1, pp. 118–128. 1938).

Campbell, Åke. *Keltisk och nordisk kultur in möte på Hebriderna* (Folk-Liv, 7–8, pp. 228–252. Stockholm, 1943–44).

Čeremnych, M. *L'essor économique et culturel de la région autonome des Khakasses* (In Russian with French résumé) (SE, 1, pp. 60–72, 1938).

Charko, L. *Un fragment de frise représentant des géants trouvé au village de Tamanskaïa* (In Russian with French résumé) (SA, 7, pp. 81–93. 1941).

Čičerov, V. *Le folklore des peuples de l'URSS* (In Russian) (SE, 1, pp. 210–222. 1938).

Čitaja, G. *L'ethnographie de la Géorgie dans le période soviétique* (In Russian) (SE, 3, pp. 207–213. 1940).

Cole, Fay-Cooper. *The Peoples of Malaysia* (354 pp., illus., maps. $4.00. D. Van Nostrand Co., New York. 1945).

Datta-Majumder, Nabendu. *Rural Survey in Travancore* (Science and Culture, 2, No. 9, pp. 440–443. 1937).

Primitive Economics (Calcutta Review, November, pp. 36–41. 1944).

Primitive Production in North East and Central India (Science and Culture, 10, pp. 72–78. 1944–45).

Exchange, Distribution and Consumption in Aboriginal India (Calcutta Review, January, pp. 17–21. 1945).

Property in Aboriginal India (Calcutta Review, May, pp. 53–58. 1945).

Deržavin, N. *L'albanologie et le problème de l'origine de slaves méridionaux* (In Russian with French résumé) (SE, 3, pp. 185–200. 1940).

Emsheimer, Ernst. *Zur Ideologie der lappischen Zauber Zaubertrommel* (Ethnnos, 9, Nos. 3–4, pp. 141–169. Stockholm, 1944).

Erixon, Sigurd. *Åkerherrar och jordintressenter* (Folk-Liv, 7–8, pp. 207–227. Stockholm, 1943–44).

Kupolvalv i överkragningsteknik (*Ibid.*, pp. 200–206).

Folklivsforskningen som universitetsämne (*Ibid.*, pp. 286–313).

Gajdukevič, V. *Une villa rustica fortifiée sur la Montagne Têmir* (In Russian with French résumé) (SA, 7, pp. 45–59. 1941).

Ginsburg, V. *Les Yaghnobiens au point de vue anthropologique* (In Russian with French résumé) (SE, 3, pp. 89–91. 1940).

Granlund, John. *Lindbast och träbast* (Folk-Liv, 7–8, pp. 166–199. Stockholm, 1943–44).

Grigorïev, G. *La fabrication du papier par les artisans en Uzbekistan* (In Russian). (SE, 3, pp. 142–160. 1940).

Grinkova, N. *Les industries domestiques du vêtement chez les Oudmourtes de la Bachkirie* (In Russian) (SE, 3, pp. 107–141. 1940).

Hämäläinen, Albert. *Bostads-och Byggnadsskick Hos Skogsfinnarna I Mellan-Skandinavien* (Nordiska Museets Handlinger, 23. 180 pp., 183 figs. Stockholm, 1945).

Harva, Uno. *De gifta kvinnornas samfund med hithörande upptagningsriter* (Folk-Liv, 7–8, pp. 277–285. Stockholm, 1943–44).

Hasslöf, Olof. *Fiskeredskap och arbetslag* (*Ibid.*, pp. 56–78).

Hedlund, Märta. *Barnets uppfostran och utbildning i en storfamilj* (*Ibid.*, pp. 72–90).

Hjortsjö, Carl Herman, and Torsten Krakau. *Antropologiska data beträffande befolkningen i Lund under 1000–11000-och 1200 Talen* (Lunds Universitets Årsskrift, N. F. Avd. 2, 40, No. 4. Fysiografiska Sällskapets Handlingar, N. F., 55, No. 4, 43 pp., 6 figs., 10 tables. Lund, 1944).

Hobroh, Gunnar. *Kyrkogårdsmurar av timmer.* (Folk-Liv, 7–8, pp. 100–105. Stockholm, 1943–44).

Höltker, Georg. *Das herz-oder nierenförmige Ornament auf einer Vase von Mohenjo-dara* (Ethnos, 9, No. 1, pp. 1–34. Stockholm, 1944).

Homman, Olle. *Skyddsbränning.* (Folk-Liv, 7–8, pp. 106–112. Stockholm, 1943–44).

Iernstedt, H. *Une statuette de Dionysos trouvée a Tyritaké* (In Russian with French résumé). (SA, 7, pp. 174–188. 1941).

Ivanova, A. *Les sculptures tombales avec portraits des morts de Chersonèse* (In Russian with French résumé) (*Ibid.*, pp. 107–120).

Izikowitz, K. G. *The Community House of the Lamet* (Ethnos, 8, Nos. 1 and 2, pp. 19–60. Stockholm, 1943).

Quelques notes sur le costume des Puli-Akhâ (Ethnos, 8, No. 4, pp. 133–152. Stockholm, 1943).

Kapošina, S. *Squelettes à posture Ramasèe dans les sépultures d'oblie et de Chersonèse* (In Russian with French résumé) (SA, 7, pp. 161–173, 1941).

Karasev, A. *Sur l'alimentation en eau d'Olbie* (In Russian) (SA, 7, pp. 129–139. 1941).

Karlbeck, Orvar. *Notes on the Wares from the Chiao Tso Potteries* (Ethnos, 8, No. 3, pp. 81–95. Stockholm, 1943).

Karlgren, Bernhard. *The Date of the Early Dong-so'n Culture* (BMFEA, 14, pp. 1–28. 28 pls. 1942).

Some Ritual Objects of Prehistoric China. (*Ibid.*, pp. 65–69. 4 pls.).

Glosses on the Kuo Feng Odes (*Ibid.*, pp. 71–247).

Some Early Chinese Bronze Masters (BMFEA, 16, pp. 1–24, 24 pls. 1944).

Glosses on the Siao Ya Odes (*Ibid.*, pp. 25–169).

The Book of Odes, Kuo Feng and Siao Ya (*Ibid.*, pp. 171–256).

The Book of Odes. Ta Ya and Sung (BMFEA, 17, pp. 65–99. 1945).

Some Weapons and Tools of the Yin Dynasty (*Ibid.*, pp. 101–144, 40 pls.)

Keesing, Felix M. *Cultural Trends in the Philippines* (Far Eastern Quarterly, 4, No. 2, pp. 102–108. 1945).

Knipovič, T. *Sur l'histoire de la céramique d'art dans le nord de la région de la Mer Noire* (In Russian with French résumé) (SA, 7, pp. 140–151. 1941).

Koppers, W. *The Kolis in North-West Central India* (Ethnos, 8, Nos. 1 and 2, pp 1–18. Stockholm, 1943).

Krovčenko, I. *Les proverbes et les devinettes des Kalmyks* (In Russian) (SE, 3, pp. 177–184. 1940).

Kričevskij, E. *Les "plateformes" de Tripolje* (In Russian with French résumé) (SA, 6, pp. 20–45 1940).

Kricheli, A. *L'étude des Juifs géorgiens* (In Russian) (SE, 3, pp. 216–218. 1940).

Krieger, Herbert W. *Races and Peoples in the Philippines* (Far Eastern Quarterly, 4, No. 2, pp. 95–101. 1945).

Lessing, F. D. *Lamaist Painting Representing an Epiphany of Tsong-kha-pa* (Ethnos, 7, Nos. 2 and 3, pp. 71–78. Stockholm, 1942).

Lindblom, Gerhard. *Fischfang mit einem weissen Brett in China and ähnliche Methoden aus anderen Teilen der Welt* (Ethnos, 8, No. 3, pp. 115–132. Stockholm, 1943).

Lingis, Juozas. *Åkerns beredning i östra Litauen* (Folk-Liv, 7–8, pp. 143–165. Stockholm, 1943–44).

Lipskaja, N. *Compte-rendu préliminaire sommaire d'une mission ethnographique chez les Goldes* (In Russian) (SE, 3, pp. 250–255. 1940).

Makalatija, S. *Traits de la vie populaire des Pachaves dans le passé* (In Russian) (SE, 1, pp. 98–117. 1938).

L'ethnographie et les études régionales en Géorgie (In Russian). (SE, 3, pp. 214–215. 1940).

Mancevič, A. *Sur les ceintures scythiques* (In Russian with French résumé) (SA, 7, pp. 19–30. 1941).

Manker, Ernst. *Lapsk Kultur vid Stora Lule Älvs Kälisjöar* (Nordiska Museet. Acta Lapponica, 4. 280 pp., 403 pls., 3 maps. Stockholm, 1944).

Marti, V. *Nouvelles données sur l'industrie poissonnière au Bosphore Cimmérien d'après les fouilles de Tyritaké et de Myrmékie* (In Russian with French résumé) (SA, 7, pp. 94–106. 1941).

Marti, J. *Les stèles funéraires de l'époque hellénistique Tardive au Bosphore comme document d'histoire de la culture* (In Russian with French résumé) (SA, 7, pp. 31–43. 1941).

Matsson, Nils Hjalmar. *Dammslåtter och slåtterdammar* (Folk-Liv, 7–8, pp. 19–37. Stockholm, 1943–44).

Matzulevič, L. *Qui était callisthène nommé dans l'inscription découverte à Kertch en 1894?* (In Russian with French résumé) (SA, 7, pp. 61–80. 1941).

Nilsson, Martin P:n. *Årstidernas begynnelsedagar* (Folk-Liv, 7–8, pp. 273–276. Stockholm, 1943–44).

Nyman, Anders. *Hos den siste saxsmeden i Lerbäck* (*Ibid.*, pp. 128–134).

Opler, Morris Edward. *Japanese Folk Belief Concerning the Snake* (SJA, 1, No. 2, pp. 249–259. 1945).

Popov, A. *Extraits du compte-rendu d'une mission chez les Nganassanes organisée par l'Institut d'Ethnographie de l'Academie des Sciences de l'URSS* (In Russian with French résumé) (SE, 3, pp. 247–249. 1940).

Prokoflev, G. *L'ethnogonie des peuples du bassin de l'Ob et de l'Lénissèi* (In Russian with French résumé) (SE, 3, pp. 67–76. 1940).

Schmidt, W. *Das Himmelsopfer bei den innerasiatischen Pferdezüchtervölkern* (Ethnos, 7, No. 4, pp. 127–148. Stockholm, 1942).

Schotte-Lindsten, Ann-Sofi. *Torghandel, marknader och mötesdagari Linköping* (Folk-Liv, 7–8, pp. 113–126. Stockholm, 1943–44).

Schoultz, Gösta von. *En sydsvensk bostadsvana i etnologiskbelysning* (*Ibid.*, pp. 38–55).

Sirén, Osvald. *Chinese Sculptures of the Sung, Liao and Chin Dynasties* (BMFEA, 14, pp. 45–64, 12 pls. 1942).

Smirnov, V. S. *Le tissage ornamental russe* (In Russian) (SE, 3, pp. 92–106, 1940).

Spencer, Robert F. *The Annamese Kinship System* (SJA, 1, No. 2, pp. 284–310. 1945).

Stebnickij, S. *Les Numylanes-Aliutores. Sur les origines de l'élevage du renne chez les Koriaks méridionaux* (In Russian with French résumé) (SE, 1, pp. 129–144. 1938).

Autobiographies de Nymylanes (*Koryaks*) (In Russian) (*Ibid.*, pp. 76–79).

Stepanov, P. *Les Mordoves de Saratov. Bref aperçu historique des recherches* (In Russian) (SE,1, pp. 206–210. 1938).

Suchareva, O. *Les cérémonies nuptiales des Tadjiks de Samarkand et de quelques autres régions de l'Asie Centrale* (In Russian) (SE, 3, pp. 172–176. 1940).

Svärdström, Svante. *Tidiga dalmålningar av Kers Erik Jönsson* (Folk-Liv, 7–8, pp. 91–99. Stockholm, 1943–44).

Svensson, Sigfrid. *Bondepraktikan och den folkliga traditionen* (*Ibid.*, pp. 1–18).

Tichonickaja, N. *Le jeu populaire russe "Nous avons semé le millet"* (In Russian with French résumé) (SE, 1, pp. 145–166. 1938).

Tirén, Karl. *Die Lappische Volksmusik* (Nordiska Museet: Acta Lapponica, 3, 236 pp., 536 ex., 9 pls. Stockholm, 1942).

Vasilïev, B. *Les procédés de chasse anciens des Orotches de la région de Pacifique* (SE, 3, pp. 161–171. 1940).

Vasilevič, G. *Autobiographies de Toungouses* (In Russian) (SE, 1, pp. 73–75, 1938).

Wickström, Lars. *Roddarmadammer och drevtillverkning på Djurgården* (Folk-Liv, 7–8, pp. 253–259. Stockholm, 1943–44).

Zelenin, D. *La vie des Mordoves avant la révolution* (In Russian) (SE, 1, pp. 80–97. 1938).

Les peuples de l'extrême nord après la grande révolution socialiste d'Octobre. (In Russian with French résumé) (SE, 1, pp. 15–52. 1938).

La communauté historique de la culture des peuples russe et ukrainien (In Russian with French résumé) (SE, 3, pp. 23–34. 1940).

Zograph, A. *Trouvailles de monnaies aux emplacements de sanctuaires antiques présumés dans la région de la Mer Noire* (In Russian with French résumé) (SA, 7, pp. 152–160. 1941).

Etnografiska Avdelningen Berättelse för år 1941, 1942, 1943 (Göteborgs Museum, Göteborg).

Communication faite par l'anthropologue américain A. Hrdlicka à l'Institute d'Ethnographie de l'Académie des Sciences de l'URSS (In Russian) (SE, 3, pp. 256–258. 1940).

Africa

Balodis, Francis. *Die "Ka-" und "Ba-" Darstellung in der ägyptischen Kunst* (Ethnos, 9, No. 2, pp. 67–83. 1944).

Borgonjon, P. F. Fr. *De Besnydenis by de Tutshiokwe* (Aequatoria, 8, No. 2, pp. 59–74. Belgian Congo, 1945).

Bourdillon, Bernard. *The Future of Native Authorities* (Africa, 15, No. 3, pp. 123–128. London, 1945).

Caeneghem, R. Van. *Pour plus d'étude des valeurs indigènes Luba* (Aequatoria, 8, No. 2, pp. 78–79. 1945).

Cory, Hans, and M. M. Hartnoll. *Customary Law of the Haya Tribe, Tanganyika Territory* (299 pp., 45 tables. International African Institute, London, 1945).

Culwick, A. T. *New Beginning* (RLIJ, 2, pp. 44–49. 1944).

Doke, C. M. *Conjunctive Writing for Bantu Languages* (RLIJ, 1, pp. 10–15. 1944).

Hopgood, C. R. *The Future of Bantu Languages in Northern Rhodesia* (RLIJ, 2, pp. 8–15. 1944).

Hulstaert, G. *Le problème des mulâtres* (Africa, 15, No. 3, pp. 129–144. London, 1945).

Ibbotson, Percy. *Federation of Native Welfare Societies in Southern Rhodesia* (RLIJ, 2, pp. 35–39. 1944).

Jeffreys, M. D. W. *La serpent à deux têtes Bamum* (Bulletin de la Société d'Etudes Camerounaises, No. 9, pp. 7–12. 1945).

Jones, N. S. Carey. *Native Treasuries in Northern Rhodesia* (RLIJ, 2, pp. 40–48. 1944).

Kagame, Alexis. *Le Rwanda et son roi* (Aequatoria, 8, No. 2, pp. 41–58. Belgian Congo, 1945).

Kock, Gösta. *Some Reflections on the Problem of the Animal-headed Gods in Egypt* (Ethnos, 8, No. 4, pp. 153–160. 1943).

Kuczynski, Robert R. *Population Movements: The Contribution of Demography to the Study of Social Problems* (RLIJ, 2, pp. 16–34. 1944).

Kuper, Hilda. *The Marriage of a Swazi Princess* (Africa, 15, No. 3, pp. 145–155. London, 1945).

Lagercrantz, Sture. *Über willkommene und inwillkommene Zwillinge in Africa* (ES, 12–13, pp. 5–292, 17 maps. 1941).

The Sacral King in Africa (Ethnos, 8, Nos. 3 and 4, pp. 118–140. 1944).

McVicar, Thomas. *Death Rites among the Waluguru and Wanguru* (PM, 18, Nos. 1 and 2, pp. 26–35. 1945).

Posselt, F. W. T. *Upengula, the Scatterer* (Rhodesian Printing & Publishing Co., Bulawayo, 1945).

Ragatz, Lowell Joseph. *A Bibliography for the Study of African History in the Nineteenth and Twentieth Centuries* (vi, 47 pp. Washington, 1943).

Sicard, Harald v. *The Bird in the Zimbawe Culture* (Ethnos, 8, No. 3, pp. 104–114. Stockholm, 1943).

Stooke, G. Beresford. *Memorandum on Native Development in Northern Rhodesia* (RLIJ, 1, pp. 15–21. 1944).

Tanghe, B. O. *Les Fur et les Furu* (Aequatoria, 8, No. 2, pp. 75–78. 1945).

Unsworth, E. G. *The Conflict of Laws in Africa* (RLIJ, 2, pp. 49–55. 1944).

Ward, Ida. *The Prize Essay Competition* (Africa, 15, No. 3, pp. 156–158. London, 1945).

Winterbottom, J. M. *Africans, European Culture and the English Language* (RLIJ, No. 2, pp. 1–7. 1944).

Oceania

Burrows, E. G. *Songs of Uvea and Futuna* (BBMB, 183. 121 pp., 60 musical figs. 1945).

Emory, Kenneth P. *South Sea Lore* (BBMSP, 36. 79 pp., 36 figs. 1944).

Höltker, George. *Zeremonial-Steinbeil von den Korugu im Wagi-Tal im östlichen Zentral-Neuguinea.* (Ethnos, 7, Nos. 2 and 3, pp. 79–83. Stockholm, 1942).

Lindblom, G. *Crescent-shaped Lime-spatulas from British New Guinea* (Ethnos, 8, Nos. 1 and 2, pp. 78–80. 1943).

Thompson, Laura. *The Native Culture of the Marianas Islands* (BBMB, 185. 48 pp., 3 pls. 1945).

Physical Anthropology

Alejo Vignati, Milcíades. *Procesos neoarticulares por doble luxación mandibular en un cráneo indígena de Lumahuaca* (RMLP, n.s. 2, pp. 49–59. 1942).

Angel, J. Lawrence. *A Racial Analysis of the Ancient Greeks* (AJPA, 2, No. 4, pp. 329–376. 1944).

Dahlberg, Albert A. *The Paramolar Tubercle (Bolk)* (AJPA, 3, No. 1, pp. 97–103. 1945).

Damon, Albert, and Francis E. Randall. *Physical Anthropology in the Army Air Forces* (AJPA, 2, No. 3, pp. 293–316. 1944).

Dobzhansky, Theodosius. *On Species and Races of Living and Fossil Man* (AJPA, 2, No. 3, pp. 251–265. 1944).

Dupertuis, C. Wesley. *The Size and Proportions of Adult Midgets* (AJPA, 3, No. 2, pp. 111–127. 1945).

Elftman, Herbert. *Torsion of the Lower Extremity.* (AJPA, 3, No. 3, pp. 255–265. 1945).

Gamble, David P. *Physical Type and Mental Characters* (AJPA, 2, No. 2, pp. 195–220. 1944).

Gates, R. Ruggles. *Phylogeny and Classification of Hominids and Anthropoids* (AJPA, 2, No. 3, pp. 279–292. 1944).

Gregory, William K., and Milo Hellman. *Revised Reconstruction of the Skull of Plesianthropus Transvaalensis Broom* (AJPA, 3, No. 3, pp. 267–275. 1945).

Krahl, Vernon E., and Gaynor Evans. *Humeral Torsion in Man* (AJPA, 3, No. 3, pp. 229–253 1945).

Lanier, Raymond R. *Length of First, Twelfth and Accessory Ribs in American Whites and Negroes; Their Relationship to Certain Vertebral Variations* (AJPA, 2, No. 2, pp. 137–166. 1944).

Lasker, Gabriel W. *Observations on the Teeth of Chinese Born and Reared in China and America* (AJPA, 3, No. 2, pp. 151–163. 1945).

Leche, Stella M. *A Method for Reproducing and Recording Contours* (AJPA, 2, No. 3, pp. 275–277. 1944).

Oetteking, Bruno. *Skeletal Remains from Prince William Sound, Alaska* (AJPA, 3, No. 1, pp. 57–96; No. 2, pp. 177–205; No. 3, pp. 277–310. 1945).

Queiroz Lopes, A., and J. A. Serra. *Contribuições para o estudo da antropologia portuguesa XXI* (Instituto de Antropologia, Universidade Coimbra, 4, No. 5, Coimbra, 1944).

Schultz, Adolph. *Age changes and Variability in Gibbons* (AJPA, 2, No. 1, pp. 1–129. 1944).

Seltzer, Carl C. *The Relationship between the Masculine Component and Personality* (AJPA, 3, No. 1, pp. 33–47. 1945).

Strandskov, Herluf H. *Plural Birth Frequencies in the Total, the "White" and the "Colored" U. S. Populations.* (AJPA, 3, No. 1, pp. 49–55. 1945).

Birth Sex Ratios in the Total, "White" and the "Colored" U. S. Populations (AJPA, 3, No. 2, pp. 165–175. 1945).

Tamagnini, Eusébio. *Contribuições para o estudo da antropologia portuguesa XXII—o indice nasal portugueses* (Instituto de Antropologia, Universidade de Coimbra, 5, No. 1. Coimbra, 1944).

Tildesley, M. L., and N. Datta-Majumder. *Cranial Capacity: Comparative Data on the Techniques of MacDonnell and Treitinger* (AJPA, 2, No. 3, pp. 233–249. 1944).

Weidenreich, Franz. *The Keilor Skull: A Wadjak Type from Southeast Australia* (AJPA, 3, No. 1, pp. 21–32. 1945).

The Paleolithic Child from the Teshik-Tash Cave in Southern Uzbekistan (AJPA, 3, No. 2, pp. 151–163. 1945).

Wiener, Alexander S., Ruth B. Belkin, and Eve B. Sonn. *Distribution of the A_1-A_2-B-O, M-N, and RH Blood Factors among Negroes in New York City* (AJPA, 2, No. 2, pp. 187–194).

Wiener, Alexander S., Eve B. Sonn, and Chien L. Yi. *Blood Groups, Sub-groups, M-N and RH Types of Chinese* (AJPA, 2, No. 3, pp. 267–273. 1944).

Wilson, Elsie A. *Basal Metabolism from the Standpoint of Racial Anthropology* (AJPA, 3, No. 1, pp. 1–19. 1945).

Prehistory

Lowe, C. van Riet. *Prehistoric Rock Engravings in the Krugersdorp-Ristenburg Area of the Transvaal.* (SAJS, 41, pp. 329–344. 1945).

Pitfalls in Pre-history (*Ibid.*, pp. 345–352).

Okladnikov, A. *L'homme néanderthalien et les vestiges de sa culture dans l'Asie Centrale* (In Russian with French résumé) (SA, 6, pp. 5–19, 1940).

Raudonikas, V. *Le cimetière néolithique sur le lac Onéga* (*Ibid.*, pp. 46–62).

Miscellaneous

Baber, Ray E. *Sociologists and the Army Language and Area Studies* (Research Studies of the State College of Washington, 13, No. 1, pp. 25–32. Pullman, 1945).

Beaglehole, Ernest. *Character Structure, Its Role in the Analysis of Interpersonal Relations.* (Psychiatry, 7, No. 2, pp. 145–162).

Boas, Franz. *Race and Democratic Society* (220 pp. $2.50. J. J. Augustin, New York, 1945).

Boggs, R. S. *Valor práctico del folklore* (AI, 5, No. 3, pp. 211–215. 1945).

Bryson, Gladys. *Man and Society: The Scottish Inquiry of the Eighteenth Century* (287 pp. $3.00. Princeton University Press, Princeton, 1945).

Collier, John. *Indianismo vs. Racism?* (AI, 5, No. 3, pp. 241–246. 1945).
Commentary on Miss Sazl, Communication (AI, 5, No. 3, p. 255. 1945).

Coomaraswamy, Ananda K. *"Spiritual Paternity" and the "Puppet-Complex." A Study in Anthropological Methodology* (Psychiatry, 8, No. 3, pp. 287–197. 1945).

Cooper, John M. *Some Recent Anthropological Publications* (PM, 18, Nos. 1 and 2, pp. 26–27. 1945).

Eissler, Kurt R. *Balinese Character* (Psychiatry, 7, No. 2, pp. 139–144. 1944).

Fodor, Nandor. *Lycanthropy as a Psychic Mechanism* (JAFL, 58, No. 230, pp. 310–316. 1945).

Fuente, Julio de la. *Sobre nutrición y enfermedades de Indios* (AI, 5, No. 3, pp. 235–239. 1945).

Gamio, Manuel. *El material folklórico y el progreso social* (*Ibid.*, pp. 207–210).

Gillin, John. *Custom and Range of Human Response* (CP, 13, No. 2, pp. 101–134. 1934).

Gluckman, Max. *The Difficulties, Achievements, and Limitations of Social Anthropology* (RLIJ, 1, pp. 22–43. 1944).

Gottschalk, Louis, Clyde Kluckhohn, and Robert Angell. *The Use of Personal Documents in History, Anthropology and Sociology.* ($1.50. Social Science Research Council, New York, 1945).

Havighurst, R. J., and R. R. Hilkevitch. *The Intelligence of Indian Children as Measured by a Performance Scale* (JASP, 39, pp. 419–433. 1944).

Honigmann, J. J. *Morale in a Primitive Society* (CP, 12, pp. 228–236. 1944).

Hornell, James. *Balancing Devices in Canoes and Sailing Craft* (Ethnos, 10, No. 1, pp. 1–16. 1944).

Kardiner, Abram, with collaboration of Ralph Linton, Cora DuBois, and James West. *The Psychological Frontiers of Society* (xxi, 475 pp. $5.00. Columbia University Press, New York, 1945).

Klineberg, Otto. *A Science of National Character* (AmS, 32, pp. 273–285. 1944).

Kluckhohn, Clyde. *Group Tensions: Analysis of a Case History* (In "Approaches to National Unity," Fifth Symposium, pp. 222–243. New York, 1945).

La Barre, Weston. *Some Observations on Character Structure in the Orient. The Japanese.* (Psychiatry, 8, No. 3, pp. 319–342. 1945).

Lagercrantz, Sture. *Inflated Skins and their Distribution* (Ethnos, 9, No. 2, pp. 49–66. Stockholm, 1944).

Lindblom, Gerhard. *Obituary of Ida Trotzig* (Ethnos, 9, No. 1, pp. 41–42. Stockholm, 1944).
An Ethnological Exhibition (*Ibid.*, pp. 46–48).

Linné, S. *Humpbacks in Ancient America* (Ethnos, 8, No. 4, pp. 161–186. Stockholm, 1943).
Primitive Rain Wear (Ethnos. 9, Nos. 3 and 4, pp. 170–198. Stockholm, 1944).

Mead, Margaret. *Cultural Approach to Personality; Anthropological Comment on the Frame of*

Reference of Andras Angyal (Transactions of the New York Academy of Sciences, 6, pp. 93–101. New York, 1944).

Murdock, George P., Clellan S. Ford, and others. *Outline of Cultural Materials* (YAS, 3. vii, 56 pp. New Haven, 1945).

Osborne, Lilly de Jongh. *Doctor Robert James Burkitt* (ASGH, 20. No. 2, pp. 95–96. 1945).

S. L. *Obituary of Axel Hjelm* (Ethnos, 9, No. 1, pp. 44–45. Stockholm, 1945).

Obituary of Ola Apenes (*Ibid.*, pp. 43–44).

Salz, Beate. *Some Considerations on Mr. Collier's Article* (AI, 5, No. 3, pp. 247–254. 1945).

Stevens, W. L. *Estimação estatistica* (137 pp. Universidad of Coimbra, Coimbra, 1944).

Struve, V. *La définition de la première société de classes selon Karl Marx* (*In Russian* with French résumé) (SE, 3, pp. 5–22. 1940).

Suárez, Victor M. *La importancia de los estudios lingüísticos como conexo de las investigaciones históricas y folklóricas* (YMT, 6, No. 68, pp. 75–78. 1945).

Taylor, W. S. *A Note on Cultural Determination of Free Drawings* (CP, 13, pp. 30–36. 1944).

Uribe Escobar, Ricardo. *Los derechos de familia* (Estudios de Derecho, 6, No. 17, pp. 5–65. Medellín, Colombia, 1944).

Villa Rojas, Alfonso. *Significado y valor práctico del folklore* (AI, 5, No. 4, pp. 295–302. 1945).

Vinogradov, G. *L'ethnographie dans la sphère des intérêts scientifiques de N. Černysevskij* (In Russian) (SE, 3, pp. 35–55. 1940).

White, Leslie A. *History, Evolutionism and Functionalism; Three Types of Interpretation of Culture* (SJA, 1, No. 2, pp. 221–248. 1945).

Wohlstein, Herman. *Zu einigen Zaubermotiven in der biblischen Umwelt* (Ethnos, 9, No. 2, pp. 84–94. 1944).

BRIEF COMMUNICATIONS

A REPORT BY ABBÉ HENRI BREUIL ON HIS OBSERVATIONS OF NEW PREHISTORIC SITES IN MOROCCO, PORTUGAL AND SOUTH AFRICA

Under date of April 17, 1945, Professor Abbé Henri Breuil reported to me from Johannesburg, South Africa, about his studies on Paleolithic sites in Portugal and Africa. No doubt it will interest readers to know that the celebrated French scholar succeeded in spending some fruitful years of study while his country was ravaged by war. He says "Probably I had exceptional luck, for I was first in Portugal, then in Morocco, and now for over two years I was able to follow in relatively good circumstances my researches, especially with reference to correlations between former sea-levels and Paleolithic cultures. As for the problems which may interest you, I can report some of the new results. . . . "

In Morocco, R. Neuville and A. Rühlman discovered at the end of 1940 a splendid stratigraphic sequence illustrated by the general section (Fig. 1). The beaches represent

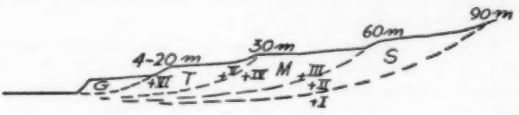

FIG. 1. Cross Section through culture-bearing beach deposits in Morocco. Letters refer to stages mentioned in text; numerals indicate sequence of Stone Age cultures, with Vi marking youngest of industries found.

the Sicilian, Milagyan, Tyrrhenian and Grimaldian stages, each of them being covered in the outcrops of beach pebbles by calcareous deposits. These represent consolidated dunes, already hardened prior to a succeeding rise of sea-level, found associated with wave-cut caves lying at corresponding levels. In the Abdu Rahman diggings which are about 500 m. long, were found: (1) some pebble implements and two handaxes (Abbevillian type) worn by sea water; (2) directly over the beach deposits, a bone breccia, mainly teeth, with Clacton-Abbevillian implements; (3) in a thin gravel of fresh-water origin, a small-flake industry; (4) on the Milagyan beach, another layer with small flakes; (5) a deposit with water-worn implements of Abbevillian type apparently washed out in the post-Milagyan stage during which the sea retreated, permitting occupancy of the corresponding caves by Acheul people. The Tyrrhenian beach is better exposed at Marlin and Eltarek (?), some seven kilometers distant from Abdu Rahman, its gravel being full of neo-Abbevillian types of unknown origin.

The Acheul people here camped on the beach and, in extracting stones, their unworn implements became mixed with the former. They must have occupied the site for a long time. Their camp sites are overlain by layers containing axes of finer workmanship, suggestive of Levallois technique.

In Portugal the composition of the beaches is quite different. The sea here never entered far inland. The Grimaldian stage of marine regression (Würm Phase), except in the north, is present only in the form of patches of conglomerate battered by wave action. In the wave-cut cave of Fumischa occurs a warm fauna with Mousterian, but elsewhere Languedocien and Ancorien (?) industries, the former characterized by

discoid pebbles, chipped all around the edges, the second by picks resembling the postglacial Asturian industries. The Ancorien (?) is also found lower than the Milagyan level.

The Tyrrhenian stage (Mindel-Riss) is very constant and well developed. At the base it contains Acheul implements in a worn condition. The post-Tyrrhenian industries are well developed and quite unworn. The higher levels are scarcely represented; the Milagyan for instance, being of sandy composition, contains no worked stones. By contrast, the 90 m. Sicilian beach deposits are very rich in pebble implements, including a few Abbevillian hand-axes.

In South Africa the older industries are not worn and occur not higher than in 400 ft. beach levels (cf. Abbevillian), but in the lowest gravel they are rolled by waves. The later industries (cf. Upper Acheul) are found in beach deposits at levels ranging from 40 to 80 feet and are mixed with water-worn implements of the Middle Stellenbosch culture. The lower beach deposits contain various cultures of the Middle Stone Age, which in fact are found also at lower levels. I have written two notes on this particular site but have not received any reprints as yet.

Along the Vaal river the sections are well known from the diamond pits, and since the younger formations here have been thoroughly investigated I concentrated on the older, higher layers. Many times I visited the terraces exposed near Vereeniging and found in them implements of Acheul type. Two higher terrace gravels composed chiefly of quartzite contain enormous quantities of implements of Acheul-Abbevillian type, sometimes water-worn and sometimes not. The 100-foot terrace contains many flakes and cores of rather small size, and some rare handaxes of proto-Abbevillian type.

Further downstream, between Bloemhoff and Barkly and separated from the former section by rapids, the situation changes. The lower gravels contain various stages of the Stellenbosch culture (handaxes), a few Abbevillian types very much worn, extraordinary numbers of less water-worn Acheulean implements which are followed in the section by Stellenbosch Stages IV and V (hardly worn), and on top of this a reddish sandy deposit with Fauresmith culture (cf. Mousterian or traditional Acheul). The latter is made of indurated shale and not of dolerite (trap-rock) as are the former industries, because the filling of the stream bed covered the dolerite-bearing gravels. Still higher along the valley slopes, at about 200 feet above stream level, occur gravels composed of quartzite, quartz, agath with water-worn pebble implements and less worn flakes and cores, and, more rarely, handaxes.

The remarkable thing is that there are here no differences in the industries appearing in the 200, 150, 60–80-foot levels. In deeper pits and diggings we noticed a coarse conglomerate (potato gravel) resting on an older stream gravel composed of basaltic rocks cemented with lime and free from implements. This discovery suggested that those various levels are not stream-laid but faulted portions of a sequence sliced and dropped downward successively by faults. In fact I believe that the later downcutting of the Vaal river, between the 50-foot terrace and the lowest gravels, was occasioned by the latest Rift Valley movements, forcing the Vaal stream to lower its bed and to break through various barriers of dolerite rock. These erosion stages along the Vaal river presumably took place under pluvial conditions.

The latest of these stages is certainly as young as the Upper Middle Stone Age. In the Makayon caves (Northern Transvaal) this culture is covered by thick stalagmite

deposits. Therefore the later Stone Age cultures should be more or less of the same period as our European Meso- and Neolithic.

I believe that Leakey and Van Riet-Lowe are right in assuming that the glaciations of the northern hemisphere were synchronous with the Pluvial phases of this country, which would make the Interglacial periods correspond with Dry phases.

The Middle Stone Age with its coastal beaches of 20 feet began in the Riss-Würm interglacial and these were succeeded by deposits of Würm age. The Stellenbosch IV–V follows the formation of the beach lying at 40–80-foot levels (cf. Tyrrhenian or Mindel-Riss). The Middle Stellenbosch is earlier, the older Stellenbosch (beach of 400 feet) is Sicilian and older, and the pebble cultures perhaps earlier than the Sicilian.

With the discovery of large fossil apes at Taungs, Sterkfontain, Kromdrai, all of which appear to have been in the line of human evolution, and considering that Broom now believes them to be of Pliocene age rather than Pleistocene, the probability of very ancient human fossils in South Africa has become very strong.

As for the Wet and Dry periods, the Middle Stone Age of the hill countries, generally speaking, appears associated with ferruginous deposits, while in the lowlands they occur in dune formations. These show four distinct zones of black earth, similar to the younger loess of Europe.

In the East Orange Free State, bordering on Basuto Land, early Middle Stone Age cultures appear in the oldest stream deposits of the first erosional stage. Here the second erosion exposed the basal gravels containing rolled early Middle Stone Age implements."

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NATCHEZ SOCIAL STRUCTURE AS AN INSTRUMENT OF ASSIMILATION

In a recent article[1] C. W. M. Hart has expressed the point of view that the social organization of the Natchez as summarized by Swanton is actually a biological impossibility.

As outlined by Swanton[2] Natchez society was divided into two broad classes: aristocracy and commoners. The aristocracy, composed of Suns, Nobles, and Honored People, was exogamous. All members of the aristocracy had to marry commoners. But the commoners, who were called Stinkards, could marry within their own class or into the aristocracy.

The rank of children born of such marriages was bestowed according to two different principles. The first principle was direct matrilineal descent of rank. When a Sun woman married a Stinkard, the children were Suns; when a Noble woman married a Stinkard, the children were Nobles; when an Honored woman married a Stinkard, the children were Honored People; and when a Stinkard woman married a Stinkard, the children were Stinkards.

The second principle was indirect patrilineal descent of rank. When a Sun man married a Stinkard, the children were Nobles; when a Noble man married a Stinkard, the children were Honored People; when an Honored man married a Stinkard, the

[1] Hart, 1943. [2] Swanton, 1911.

children were Stinkards; and when a Stinkard man married a Stinkard, the children were also Stinkards.

Hart[3] has aptly demonstrated that the operation of this system would provide a constant drain on the Stinkards in favor of the aristocracy. With population relatively constant the Stinkards eventually would be assimilated completely by the aristocracy and the social system would break down. As an hypothesis Hart[4] presents a modification of the Natchez system which would remedy the defects he has demonstrated.

It is my desire to present another point of view which, although dependent upon Hart's analysis and demonstration, is nevertheless independent of his resultant hypothesis. In view of Hart's demonstration it seems obvious that the Natchez system is an impossibility for a relatively constant population. It is my thesis, however, that the Natchez population was not relatively stable. Rather the population was increased by the assimilation of surrounding peoples. And when such assimilation occurred the new additions were Stinkards.

There is some historical evidence in support of this hypothesis. At the time of Du Pratz (1718–30) the Tunican-speaking Tioux "lived in a small village among the Natchez who had adopted them, though only into the lowest or Stinkard class."[5] The Tioux were remnants of a once powerful group who had been badly defeated by the Chickasaws.

Another foreign group adopted as Stinkards by the Natchez was the Tunican-speaking Grigra.[6] Like the Tioux they lived in a small village among the Natchez, or by virtue of their adoption they constituted one of the Natchez villages. Du Pratz[7] stated that the Natchez adoption of the Grigra was earlier than that of the Tioux, therefore, presumably before the period 1718–30. There may have been other groups assimilated into the Stinkard class of the Natchez, but the two documented examples cited above will suffice for the argument.

Thus assuming that the Natchez system as described by Swanton[8] is essentially correct it could only operate in terms of an inexhaustible supply of Stinkards. The supply of Stinkards must have been maintained in considerable part by the assimilation of non-Natchez peoples.

This hypothesis of assimilation does not actually remedy the defects of the Natchez system. It merely postpones indefinitely the time when the system would fail completely. And although the Natchez were approaching such a time, I do not believe they had reached it for two reasons.

One reason is factual, the other hypothetical. The factual reason already has been stated: the Natchez were assimilating foreign populations via the Stinkard class. The other reason is that the Natchez system probably was not ancient.

Kroeber[9] suggests that the Natchez system represents the culture climax of aboriginal eastern North America, and is a remnant from an earlier period. I do not question that this system was a climax, but I believe that the early French explorers were seeing this climax in operation. If the Natchez system were ancient I would expect to find some evidence of it among other tribes of the Lower Mississippi Valley,

[3] Hart, 1943. [4] Hart, 1943. [5] Swanton, 1911, p. 334.
[6] Swanton, 1911, p. 336. [7] Cited by Swanton, 1911, p. 336.
[8] Swanton, 1911. [9] Kroeber, 1939, p. 62.

who, archaeologically, were descended from the same cultural stratum as the Natchez.

The Natchezan archaeological culture[10] is not old nor is it a climax. Most of the sites are of the period 1680–1740 A.D. Furthermore, stratigraphic evidence and typological evidence indicate that the Natchezan archaeological culture was at least in part a development from the Plaquemine culture which was earlier.

The Plaquemine culture which was a continuum of Coles Creek can be dated in two different ways. Plaquemine, for instance, cross-dates with Caddo cultures of northwestern Louisiana and Texas which do not seem to be earlier than 1500 A.D.[11] (Krieger, 1945). These particular Caddo cultures have been dated by means of trade sherds of Pueblo glaze wares.

Moreover, it was either Plaquemine or late Coles Creek which was encountered by De Soto near the mouth of the Ouachita River in Louisiana about 1540 A.D. It thus seems almost certain that the Plaquemine culture is associated with a period circa 1500 A.D. or later. Since the Natchezan archaeological culture is later than Plaquemine, the former most probably did not exist until circa 1600 A.D.

Assuming, however, that Natchezan archaeological culture existed in 1600 A.D. and that the Natchez social system is correlated directly with it, the Natchezan social system would have had a life span of about 150 years. Allowing five generations to the century (a generous allowance), there would have been about seven generations in which the Natchez social system operated. Despite the defects of this system it seems reasonable to believe that it could have maintained itself for seven generations, especially with opportunities to add to the Stinkard population by means of assimilation.

Probably some process of assimilation is reflected in the Natchezan archaeological culture which shows both Caddo and Choctaw influences. The traits indicative of the latter are nevertheless an integral part of the Natchezan archaeological culture. There undoubtedly was some assimilation of Tunica traits, but unfortunately there is now little certainty as to what archaeological complex constituted the main body of the Tunica. (The Natchezan archaeological culture[12] embraced such peoples as the Natchez, Avoyel, Taensa, Bayogoula or Quinipissa, and some Houma and Tunica.)

There are both documentary and archaeological evidence that the Natchez assimilated foreign elements. Thus the hypothesis that an imperfect social system was temporarily maintained by assimilation of outsiders does not seem unwarranted, especially if this social system had not long been in operation.

In this paper I have merely expressed an opinion based upon facts and interpretations of archaeology and ethno-history. It is doubtful that this opinion (or these opinions) could ever be proved.

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BIBLIOGRAPHY

HART, C. W. M.
1943 *A Reconsideration of the Natchez Social Structure*. American Anthropologist, Vol. 45 No. 3, pp. 374–386.

KRIEGER, ALEX
1945 Letter of May 24.

[10] Quimby, 1942. [11] Krieger, 1945. [12] Quimby, 1942.

KROEBER, A. L.
1939 *Cultural and Natural Areas of North America*. University of California Publications in American Archaeology and Ethnology, Vol. 38.

QUIMBY, G. I.
1942 *The Natchezan Culture Type*. American Antiquity, Vol. 7, No. 3, pp. 255–275.

SWANTON, J. R.
1911 *Indian Tribes of the Lower Mississippi Valley and Adjacent Coast of the Gulf of Mexico*. Bureau of American Ethnology, Bulletin 43.

LEADERSHIP AT NAVAJO MOUNTAIN AND KLAGETOH

In a recent article[1] Dr. Goldfrank brought out some suggestive ideas on the subject of Navaho leadership. Her thesis is that community organization and leadership are related to the development and maintenance of irrigation. The principal examples cited are from my material on the Navajo Mountain and Klagetoh areas. She concludes that leadership is amorphous at Navajo Mountain because that area was first occupied by Navahos for herding, whereas at Klagetoh leadership is strong because of a longer agricultural history.

There is certainly a marked apparent difference in leadership in these two areas. This might be summarized by saying that, as far back as memory serves, the Klagetoh people have been conscious of the office of leader, while the Navajo Mountain people have been conscious of leadership as a function rather than as a formal, recognized office. This contrast holds for the periods during which these areas were settled and up to the present.

What has been the relative importance of herding and farming at Navajo Mountain and Klagetoh? Dr. Goldfrank emphasizes the point that because Navajo Mountain was settled in the hey-day of pastoralism and at first was used only for winter range, it was settled on a pastoral basis. But while these particular people were being pastoral at Navajo Mountain in the winter, they were also being agricultural to the south in the summer. Also, while the first farms were being developed at Klagetoh after the return from Fort Sumner, these very farmers were receiving sheep and herding them in the Fort Defiance area. That is, although the Navajo Mountain and Klagetoh *areas* may have been initially pastoral and agricultural respectively, the people who settled them were both herders and farmers.

As reported in 1938,[2] Navajo Mountain and Klagetoh received 34% and 35% respectively of their income from livestock, against an average for the reservation of 29%; and 26% and 22% respectively from agriculture with an average of 22% for the reservation. Thus in these respects they are similar to each other and quite typical of the reservation.

The important contrast between Navajo Mountain and Klagetoh is in the structure of the groups occupying the two areas. The central factors at Navajo Mountain are the high degree of interrelationship through kinship and the extreme isolation of the group

[1] Esther S. Goldfrank, *Irrigation Agriculture and Navaho Community Leadership: Case Material on Environment and Culture*. American Anthropologist, Vol. 47, No. 2, pp. 262–277.

[2] *Statistical Summary Human Dependency Survey, Navajo and Hopi Reservations*. Soil Conservation Service, Section of Conservation Economics, Region 8, 1938.

from other Navaho. Neither extreme interrelatedness nor isolation is true of the Klagetoh group. This contrast affects the role of leadership in the two areas. At Navajo Mountain many controls operate on a basis of clan or blood relationship since these relationships are so widespread in the group. At Klagetoh many controls are on a non-kin basis, so that controls other than those based on clan or blood relationship must operate and hence the position of leader is more crystallized.

These points do not disprove all connection between irrigation agriculture and development of leadership among the Navaho. But the special circumstances found at Navajo Mountain and Klagetoh seem to me to preclude finding any proof of such a relationship in these two areas.

MALCOLM C. COLLIER

CHICAGO, ILLINOIS

THE GUINEAS OF WEST VIRGINIA

Apparently no writers have hitherto considered it important enough to discuss a strange mixed group of people who are scattered through several counties of West Virginia and who are called "Guineas." The Writers' Guide to West Virginia contains no notice of these people nor do any of the other histories and descriptions of that State, so far as I have been able to ascertain, with one exception. (Hu Maxwell. *The History of Barbour County, West Virginia.* Morgantown. 1899. pp. 310–311.)

The Guineas first came to my attention a few months ago when they were casually mentioned by my good Piscataway Indian friend, Philip S. Proctor, as a mixed Indian-White group like the Wesorts of southern Maryland. He spoke of them as dwelling in several communities of West Virginia, namely Four States (Marion County), Rowlesburg (Preston County), Philippi and Belington (Barbour County) and Kerens (Randolph County). A few weeks later I inquired of my dentist, who is from West Virginia, if he had heard of any mixed Indian-Whites in that State, and he exclaimed at once, "O yes! Those are a people called 'Guineas.' "

Soon thereafter I wrote to the West Virginia State Department of Archives and History inquiring about these people. That agency referred my letter to the Public Librarian of one of the counties in that State, and she sent me a letter containing the names of several persons who might have information and mentioned a few facts that she knew about the Guineas.

In addition to the counties mentioned above, these people are said to have a large settlement in Braxton County near Sutton. Two family names are mentioned as comprising all the members of this group in that county, namely Male (pronounced to rhyme with shale) and Croston. It is said that quite recently much excitement was created in one county when a man of this group was returned from Oregon to answer a forty-year-old murder charge. In this connection it is worth while to note that in the adjacent western Maryland county of Garrett near Deer Park and Oakland, an inquiry by James Mooney in 1889 elicited the fact that mixed Indian-White families bearing the name of Male or Mail were to be found.

At Philippi the Guineas live on Chestnut Ridge above the town. This manner of dwelling on ridges recalls the Melungeons of Newman's Ridge in Virginia and Tennessee (Hancock County, Tennessee, and Lee County, Virginia). Yet, Mr. Bruce Crawford,

Director of the Highway Safety Bureau of West Virginia State Police, informs me that the Guineas cultivate gardens and engage in kinds of work which the Melungeons only in rare instances engage in. Hence it is questionable whether the Guineas are derived from the Melungeons or have any relationship to them. This relationship might be more thoroughly studied through a complete list of Guinea family names and comparison with those of the Melungeons.

In some areas the Guineas are said to have their own schools and churches (Protestant) while elsewhere they are said to attend White institutions of this sort. Many of the local White people seem to regard them as part Negro in spite of the fact that some are quite indistinguishable from Whites. Guineas are said to be frequently employed in the manufacture of illicit liquor or "mountain moonshine." Some are said to find employment as domestic servants. Others are miners like the remainder of the population in communities where this occupation predominates. These people are said to dislike greatly the sobriquet given them by their White neighbors, "Guineas," yet I have heard of no alternative designation than this one. It is to be hoped that future investigation will elucidate the status and background of this hitherto unnoticed minority group in the West Virginia population.

WM. H. GILBERT

LIBRARY OF CONGRESS
WASHINGTON, D. C.

THE NAME OF THE NAMBIKUARA

In a joint communication published in a recent number of this journal[1] Messrs. Emilo Willems and Egon Schaden have taken exception to some transcriptions of tribal names, location of tribes and linguistic affiliations proposed by Dr. Steggerda.[2] While some of their comments are relevant and others not, as pointed out by the Editor, the entire note displays a pedantic approach to the problem of tribal names which should be dismissed once and for all, especially in relation to South America. The best example is provided by the name of the *Nambikuara*.

The authors blame Dr. Steggerda for spelling it *Nambiquara* or *Nambikuara;* they punctiliously emphasize that it is not a Spanish word but Tupi and that it ought to be written *Nhambikwara*. Since it is only a nickname of foreign origin and entirely unfamiliar to the natives, one could think that the question of spelling is not of the utmost importance. But there is more to the point.

The name *Nambikuara* was mentioned for the first time, and only by hearsay, by Antonio Pires de Campos in the beginning of the 18th century. Since then it appeared several times with reference to an unknown tribe located on the headwaters of the Tapajoz. There is a great discrepancy between the different spellings. When General (then Colonel) Candido Mariano da Silva Rondon began to explore the land between the Tapajoz and the Gi-Paraná, in 1907, he met with an unknown group speaking several dialects of an unknown language, and he did not hesitate to identify them with the tribe often mentioned in the early documents. It was at that time that the name

[1] Emilio Willems and Egon Schaden, *Stature of South American Indians*, American Anthropologist, vol. 47, no. 3, July-September 1945; pp. 469–470.

[2] American Journal of Physical Anthropology, I, no. I, 1943.

Nhambikwara was definitely adopted, its spelling fixed, and that it was recognized as a Tupi nickname the meaning of which is "big ears."

Some anthropologists remained vaguely disturbed, however, as the ears of the *Nambikuara* are not particularly big, and they do not use any of the ornaments worn in some other parts of Brazil with the result of enlarging the ear-lobe. The Tupi nickname would suggest, of course, some conspicuous custom of the kind.

Such was the state of things when, during my 1938–39 expedition to the *Nambikuara* my attention was called to a tribe never mentioned before, but of which the few dwellers of these otherwise uninhabited regions were well aware, as they raided and partly destroyed the telegraphic post of Paressi a few years ago; other clashes with gold-diggers and rubber-tappers venturing too far inland have also occurred. I did not find a chance to see the natives, but their campfires were getting closer and closer while I was there and the Brazilian dwellers of the border were becoming increasingly worried. I was shown the weapons they had abandoned on retiring from their recent raid together with a few other items, none of which could be attributed to the *Nambikuara* culture.

Besides, there are two interesting facts: the tribe's territory is close to the *Nambikuara* but it is situated more to the East. This means that any expedition coming from the Cuiabá region (as was the case for 18th century travellers) should have approached them before reaching the *Nambikuara*. Furthermore, present-day adventurers in search for gold, diamonds or rubber who happened to see these much-dreaded natives, call them *beiços de pau*, a Portuguese expression which means "wooden mouths." They all agree that the natives wear wooden discs in the ear-lobes as well as in the lower lip, whence the designation now commonly used in the *sertão* Northwest of Cuiabá. Therefore I submit that this tribe is the "big ears" of the Tupi and of the older literature, whose nickname was mistakenly transferred to the *Nambikuara* at the beginning of the 20th century because, at that time, no other tribe was known to dwell in that particular region. The name *Nambikuara* is not only a surname of foreign origin; it was not even intended for the people who will probably be labeled as such forever.

To de-christen the modern *Nambikuara* would only bring more confusion since they have now been called that for almost forty years. A new name should be artificially created, for they only have names for their different bands and do not use any general appellation for the linguistic group taken as a whole. Thus the reasonable solution points to retaining the present name of the *Nambikuara;* at the same time, one should be aware that under these circumstances the word is absolutely meaningless, a code word of which only the phonetic spelling can give an adequate treatment.

I have related this story because it brings us a lesson, for many similar cases exist in South America and elsewhere. Tribal names should not be approached with an overdogmatic mind. There are other topics which better deserve a scholar's care and attention.

CLAUDE LÉVI-STRAUSS

ÉCOLE LIBRE DES HAUTES ÉTUDES
NEW YORK CITY

ANTHROPOLOGY DURING THE WAR

II. Scandinavia*

The effects of the war on anthropology have been less disruptive in Scandinavia than in most other parts of Europe. In Sweden there has been scarcely any interruption of anthropological activity, and Danish anthropologists carried on their work despite the German occupation. The Committee lacks direct information on Norway and Finland, but even here, under greater handicaps and difficulties, anthropological research and publication continued to some extent. This report will summarize the available information on the four countries in relation to personnel, museums and university departments, sientific societies, and publications.

Denmark. Universities and museums are intact except for some buildings of the Aarhus University used as Gestapo headquarters and bombed by the RAF. Th. Thomsen retired in 1940 as head curator of the Ethnographical Department of the National Museum and died in 1941. He was succeeded by Dr. Kaj Birket-Smith. Capt. Gustav Holm, who discovered and first described the Ammassalik Eskimos, died in March, 1940. At the University of Copenhagen, Dr. William Thalbitzer retired as Professor of Eskimo language and culture and was succeeded by Dr. Erik Holtved, as docent. In the fall of 1945, the University for the first time offered a regular course in Ethnology, with Dr. Birket-Smith as reader.

In Copenhagen a new Arctic society has been established: Societas Arctica Scandinavica, devoted to arctic research in the humanistic and natural sciences. When organization is completed the Society will include scientists in the other Scandinavian countries. The present Danish committee includes Professor Knud Jessen (Chairman), Dr. M. A. Degerbøl, Capt. Einar Mikkelsen, Professor A. Rosenkrantz, Dr. Therkel Mathiassen, and Dr. Kaj Birket-Smith. Two numbers of the Society's journal *Acta Arctica Scandinavica* have been issued, containing articles by Erik Holtved on the Eskimo legend of Navaranâq and by G. Gjessing, of Oslo, on the circumpolar stone age.

The Ethnographical Department of the National Museum has also begun a new series: *Nationalmuseets Skrifter, Ethnografisk Raekke*, on the occasion of the centenary of the founding of the Department. No. 1, 1941, contains a paper by K. Birket-Smith, "Early Collections from the Pacific Eskimo," and No. 2, 1944, "La Tente noir," by C. G. Feilberg.

Danish journals seem to have appeared with little or no interruption during the war, though as yet few have been received in this country. Four reports of the Fifth Thule Expedition have been issued: "Anthropological Observations on the Central Eskimos," by K. Birket-Smith, Vol. 3, No. 2, 1940; "Alaskan Eskimo Words Compiled by Knud Rasmussen," edited by H. Ostermann, Vol. 3, No. 4, 1941; "The Mackenzie Eskimos after Knud Rasmussen's Posthumous Notes," edited by H. Ostermann, Vol. 10, No. 2, 1942; "Report on the Expedition," by Therkel Mathiassen, Vol. 1, No. 1, 1945.

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* Report No. II of the Committee on International Cooperation in Anthropology, National Research Council (M. J. Herskovits, Chairman). Based on information supplied to Committee members by Dr. Kaj Birket-Smith, Dr. Sven Liljeblad, and Dr. Gerhard Lindblom.

The *Meddelelser om Grønland* has issued the second part of Thalbitzer's "The Ammassalik Eskimo," Vol. 40, 1941, and also Erik Holtved's two volume work "Archeological Investigations in the Thule District," Vol. 141, Nos. 1 & 2, 1944. The latter describes a succession of culture stages in North Greenland, and definitely establishes Dorset as the underlying culture. Another recent publication of interest to Americanists is Birket-Smith's "The Origin of Maize Cultivation," Det. Kgl. Danske Videnskab. Selskab, Hist.-Filol. Meddel., Bd. XXIX, Nr. 3, pp. 1–59, 1943.

Norway. The little information the Committee has on Norway comes mainly from Dr. Sven Liljeblad and from references in recent numbers of the Swedish journals *Fornvännan* and *Svenska Landsmål.*

The operation of the University was severely handicapped during the occupation. Some of the professors were dismissed, the University was closed temporarily, and the students refused to take the courses offered under the "new order." Professor K. E. Schreiner is about to retire as head of the Anthropological Section of the Anatomical Institute of the University of Oslo, but up to August, 1945, no one had been selected to fill his place. He is working on a second volume of his "Crania Norwegica" (Instituttet for Sammenlignende Kulturforskning, 1939), and has recently published a shorter work "Further Note on the Craniology of the Lapps." Professor Schreiner was imprisoned for a short time by the Germans and removed from his University position, though he continued to use his laboratory. Mrs. Schreiner has published another paper, "Bidrag til Rogalands Anthropologi," 1941, in her series of somatic studies on the present population of the various regions of Norway. Dr. Magnus Olsen, Professor of Old-Norse and Icelandic languages and literature at Oslo University, lectured at the University of Uppsala during the German occupation of Norway. The Rector of the University of Oslo, Dr. Didrik A. Seip, who has pronounced ethnological interests and who has given strong support to anthropology at the University, was sent to a concentration camp in Germany because of his opposition to educational changes instituted by the Quisling government. It is reported that he is now back in office.

No Norwegian anthropological publications have been received at the Smithsonian Institution or the Library of Congress since 1940. It is good to know, however, that a number of museum and society reports were issued as usual, even during the occupation. Recent numbers of *Fornvännen* and *Svenska Landsmål och Svenskt Folkliv* list the following Norwegian publications that appeared during the war years:

By og Bygd. Norsk Folkemuseums Årbok. Oslo.

Bergens Museum Årbok. Bergen.

Naturen. Bergen Museum.

Det kgl. norske videnskabers selskab. Trondheim. Skrifter, Forhandlinger, Museets Årsberetning, and *Oldsakssamlingens tilvekst.*

Stavanger Museum Aarshefte.

Viking. Tidsskrift for Norrøn Arkeologi. Oslo.

Universitetets oldsaksamling, Oslo. Årbok.

Sweden. The Committee is indebted to Dr. Sven Liljeblad of Harvard, and formerly of Lund, for a complete and detailed account of learned societies and university and museum departments concerned with anthropology in Sweden, Norway, and Finland. The following statement in Dr. Liljeblad's letter to Dr. Wm. N. Fenton shows the general situation in Sweden:

As far as Sweden is concerned, it seems as if the war and the militarization of the country have not disrupted the order in any branches of the scholarly world, and this is the case also in Ethnology except for temporary discontinuance of publication of certain periodicals with international scope. The student bodies at the universities have been unusually small because of the army mobilization; the state funds, for ethnographic field-work for instance, have been somewhat cut but not discontinued, and in certain cases small provisional funds have been given in order to support refugee scholars from other countries, who in the meantime have been active at our institutions.

Anthropological instruction has not only been continued, but expanded, at Swedish universities. Dr. Gerhard Lindblom, Director of the Statens Ethnografiska Museum, Stockholm, writes that his chair of ethnography has been made "regular" at Stockholm University, and also that his former pupil, Dr. S. Lagercrantz, has been appointed docent in the newly established chair of general and comparative ethnography at the University of Uppsala. Dr. C. W. von Sydow, holder of the first professorship in Nordic and comparative ethnology, established at Lund University in 1939, is emeritus this year and his successor has not been appointed. Dr. Walter Kaudern, director of the Ethnographical Department, Göteborg Museum, died in 1942. He was succeeded in 1944 by Dr. Karl Gustav Izikowitz who is also docent in ethnography at Göteborg University. Dr. John-Elof Forssander, Professor of prehistoric and medieval art at Lund, died Jan. 14, 1944. Dr. Knut Kjellmark, authority on the stone age in Sweden, died June 23, 1944.

The number of anthropological publications as well as museum and society reports and year books has increased since 1939. *Folk-Liv*, *Acta Ethnologica et Folklorica Europaea*, Stockholm, founded in 1937, was discontinued in 1939 but resumed publication in 1941. The following new journals have appeared since 1939:

Folkkultur, Lund.

Ethnologiska källskrifter, Stockholm. Edited by S. Erixon, H. Geijer, and Helge Nelson.

Landsmåls- och Folkminnesarkivet i Uppsala. Skrifter. Ser. A: Folkmål and *Ser. B: Folkminnen.*

Arctos Suecica, Uppsala. Edited by Oskar Lundberg.

Svio-Polonica, Stockholm.

Svio-Estonica, Lund (formerly published at Tartu University).

Finland. We have no direct information on the present status of anthropology in Finland, though university instruction and other activities doubtless have suffered during the war. Finland's leading archeologist, Dr. A. M. Tallgren, professor at the University of Helsinki and former editor of *Eurasia Septentrionalis Antiqua*, died in March, 1945, and Carl Olof Nordman, young Finnish archeologist and museum worker, was killed in action Feb. 20, 1942. Prof. Walter Anderson, folklorist at Tartu University, Estonia, went to Finland in 1940 and later to Sweden. Prof. Oscar Loortis, ethnologist at Tartu University, is now at Uppsala University in Sweden, as is Dr. Otto E. Anderson, former professor in musicology and folklore at Åbo Akedemi, Turku.

A great loss to folklore and ethnology was the discontinuation of *Folklore Fellows Communications*, published by the Academia Scientiarum Fennica, Helsinki. However, most other Finnish journals in ethnology and folklore continued publication, including the following referred to in recent issues of *Fornvännen* and *Svenska Landsmål:*

Åbo stads historiska museum. Arsskrift.
Finskt museum. Finska fornminnesföreningen.
Folkmålsstudier. Meddelanden från Föreningen för nordisk filologi i Helsingfors.
Kansatieteellinen Arkisto. (Ethnogr. arkiv. Finska fornm.-fören.)
Société Finno-Ougrienne. Mémoires.
Suomalaisen Kirjallisuuden seuran toimituksia.
Terra. Geografiska sällskapets i Finland tidsskrift.

HENRY B. COLLINS, JR.
Committee on International Cooperation in Anthropology

BUREAU OF AMERICAN ETHNOLOGY
SMITHSONIAN INSTITUTION
WASHINGTON 25, D. C.

RECOVERY OF SOME CHOUKOUTIEN MATERIAL

Dr. Frederica de Laguna has supplied the following data and press release received from her brother Wallace. They were submitted to Dr. Franz Weidenreich who approved publication with a few additional comments. In his letter to the Editor Dr. Weidenreich says that he was going to take the material with him to this country when he left Peking in the spring of 1941 but was refused official support. He further writes: "As to the discovery of the material in Tokyo, it is doubtless that the ornaments belong to the upper cave (Late Paleolithic) and not to Sinanthropus. The stone implements may also belong to the upper cave. This material was stored in my laboratory, but the implements I had were only plaster casts. So I am not sure whether the originals or plaster casts have been recovered in Tokyo. The records of the excavations from 1927 to 1937 had been taken from my laboratory where they were kept in a cabinet."

Recent reports of the recovery of the Sinanthropus skull itself in Tokyo are incorrect. The cranium there is a reconstructed replica presented by Dr. Weidenreich to the Imperial University of Tokyo in 1936.

The press release quoted below was submitted by Dr. Wallace de Laguna, U. S. Geological Survey, a member of the Natural Resources Section of the American GHQ in Tokyo when the Choukoutien material was recovered. He has added the following information, based on his own observation and on a converation with Dr. Henry Houghton, formerly attached to the Peking Union Medical College in Peking, and now in the United States.

In April, 1941, it was decided to evacuate to safety the skeletal remains of Sinanthropus from the Peking Union Medical College, where they had been deposited by the Geological Survey of China for study. Dr. Houghton and Mr. Bowen, Comptroller of the College, helped pack the bones in a small trunk. (It is not clear whether these were all the human bones, or only the most valuable specimens.) Just before Pearl Harbor, this trunk was turned over to Colonel Ashhurst, U. S. Marine Corps, for removal to the United States. The Marines were at that time being withdrawn from China, and their baggage would presumably not be subject to search. It was known that the Japanese were very anxious to secure the Sinanthropus remains and would seize any shipment leaving the College in an ordinary manner. On the 5th or 6th of

December, the equipment and effects of the Marines, including the trunk of Sinanthropus bones, were on the wharf at Chinwangtao. It is known that the baggage was captured there and it is surmised that the trunk was opened on the wharf and the contents dumped over into the water by the Japanese soldiers who were ignorant of the value of their prize.

In the summer of 1943, a Japanese scientist came to Peking to study the Sinanthropus remains and discovered that the specimens at the Peking Union Medical College were only casts, deliberately substituted for the originals in order to deceive the Japanese. The Japanese then tortured Mr. Bowen for five days in order to discover what had become of the original bones. Mr. Bowen became convinced that his captors intended to kill him, and eventually gave them what information he had. The Japanese were very angry at the Americans for the trick that had been palyed, but as far as is known were not able to recover the specimens. Dr. Houghton has since been liberated and has returned to his home in Scarsdale, N. Y., and Mr. Bowen has also been liberated.

Early in November, 1945, a letter was received by the U. S. Army Liaison Officer at GHQ, Tokyo, from the professor of Anthropology at the Imperial University in Tokyo, saying that the Japanese had taken some fossils and artifacts from Choukoutien to Japan for safe-keeping because of unsettled conditions in China, and that he now wished to return them. This letter was referred to the Natural Resources Section of GHQ for action and Dr. Frank C. Whitmore, of the U. S. Geological Survey and a member of this section, went to see the professor and took possession of the fossil animal bones, artifacts and records. There were about enough specimens to fill a small box. A subsequent inspection of all the collections of the Imperial University failed to reveal any Sinanthropus bones. While the geologists of the Natural Resources Section are keenly anxious to make every effort to find the latter, they believe that any search would prove fruitless and the full story of their fate may never be learned. Dr. Houghton is also convinced that these bones are broken and lost beyond recovery in Chinwangtao Harbor, but says that the seriousness of the loss is mitigated by the fact that at least one hundred excellent casts are deposited in learned institutions all over the world.

Dr. Wallace de Laguna, after his return to this country, briefly described to Dr. Houghton two of the large stone scrapers in the recovered collection, and the latter feels reasonably sure that these formed part of the Sinanthropus industry, an opinion which seems to be supported by the records recovered with the collection. While the specimens found in Tokyo correspond exactly with a list which accompanied them, it should be pointed out that the small size of the collection strongly suggests that it does not include all of the material excavated from Choukoutien. Furthermore, the small trunk lost at Chinwangtao probably did not contain all of the Sinanthropus remains. The fate of the rest of the Choukoutien finds is thus still unknown.

At present many staff sections of GHQ as well as the State Department are concerned with the problem of returning to the Chinese Government the recovered specimens and records. These comprise the first collection of loot to be found and returned, and the manner of return will therefore establish a precedent. The rediscovery of this famous material may become of as much importance in the field of international law and protocol as the original find was to archaeology and paleontology.

Frederica de Laguna

Press Release

More than three years after it was removed by the Japanese Government to Tokyo Imperial University, an anthropological collection including relics of one of the earliest known members of the human race has passed into the custody of the American Occupation Forces, pending its return to its owner, the National Geological Survey of China.

The collection, which was removed by Japanese scientists from Peiping Union Medical College, includes stone implements found near the skull of Peking man (*Sinanthropus pekinensis*), a sub-human which shares with the "Java ape-man" (*Pithecanthropus erectus*) the distinction of being the earliest known ancestor of man possessing human characteristics. The age of Peking man is estimated at something less than 1,000,000 years.

The presence of this valuable material in Japan first became known to the Office of the Supreme Commander for the Allied Powers through a letter from the Central Liaison Office of the Japanese Government. The Natural Resources Section of the Supreme Command was directed to take action concerning the return of the specimens, and Lt. Col. Hubert G. Schenck, Chief of the Section, dispatched Dr. Frank C. Whitmore, Jr., Scientific Consultant from the U. S. Geological Survey, to examine it.

Examination of the Japanese correspondence concerning the collection revealed that scientists of the Tokyo Imperial University visited Peiping in August 1942, at the request of the Japanese North China Army; it was at this time that the collection was brought to Tokyo.

Study of the collection reveals the presence not only of the extremely ancient implements used by Peking man, but also other tools (hide scrapers and awls), fashioned by chipping pieces of flint, chert, and quartz to a sharp edge), ornaments made from stone, fish bones, and fox and deer teeth, as well as fragments of bone and antler blackened by the fire in which the carcasses of the animals were cooked.* All these specimens came from the famous Sinanthropus cave at Choukoutien, near Peiping, a great cavity in limestone which was eaten out by running water and then, due to a change in geological conditions, was slowly filled up with 110 feet of silt and volcanic ash. It was in the lower levels of this cave that the skull cap, jaws, teeth, and other bones of Peking man were found.

Fully as important scientifically as the ancient implements, although not so spectacular, are the original research records of the excavation carried on in the Choukoutien cave from 1929 to 1937 by Dr. Davidson Black and other members of the staff of Peking Union Medical College under the auspices of the National Geological Survey of China and the Rockefeller Foundation of New York. These also were recovered from the Tokyo Imperial University. Among them is a large volume containing a plan of each level of the cave. The excavation was carried out with great precision, each level being completely cleared before excavation was begun on the next one below—a procedure which necessitated great exertion of will power on the part of the scientists, since the most valuable finds would occur at the lower levels. Each level was divided into squares, each of them numbered, and each specimen carries a number corresponding to its square, and its location is marked on the plan of the level on which it was found. Such a systematic treatment enables the scientist to trace the advances in culture undergone by succeeding inhabitants of the cave and shown by their increasing skill in fashioning tools and ornaments.

Besides the excavation plans, there were also recovered the archives and correspondence of the Cenozoic Laboratory of Peking Union Medical College, showing the development of Dr. Black's plans for the work and his study of the material discovered; annual reports of the field seasons of 1929 (when many remains of *Sinanthropus* were discovered) and 1936; the complete financial records of the work of excavation and research, 1929–1937; and a list of specimens from the cave which had been deposited in other museums, mostly in Europe and America.

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* This material belongs to the Late Paleolithic level of the "Upper Cave" of Choukoutien, and not to Sinanthropus. It may be that the stone implements mentioned are those of Sinanthropus. But I would not be surprised if they are not the originals but plaster casts, for I had those in my laboratory from which apparently all this was removed. I had no original implements in the laboratory. (Franz Weidenreich)

NOTES AND NEWS

SOCIAL SCIENCE RESEARCH COUNCIL AWARDS

The Social Science Research Council offers several classes of fellowships and awards for the academic year 1946–1947.

I. Pre-Doctoral Field Fellowships. These fellowships are open to men and women, citizens of the United States or Canada, who are candidates for the Ph.D. degree, and who will have completed prior to the end of the academic year 1945–1946 all courses and examinations for which they are eligible before completion of the thesis. The fellowships are not open to persons who will be over the age of thirty on July 1, 1946, or who plan to receive the Ph.D. before the expiration of the period of appointment for which the application is made. Appointments will be for not less than nine nor more than twelve months. The basic stipend attached to these fellowships is $1800 for a period of twelve months.

The closing date for the receipt of applications for 1946–1947 on blanks to be secured from the Secretary to the Committee is February 1, 1946. Awards will be announced April 15, 1946. When requesting application blanks, please be sure that age, academic qualifications and tentative field plans are specifically indicated. For further information, write to Miss Laura Barrett, Secretary to the Committees, 230 Park Avenue, New York 17, N. Y.

II. Post-Doctoral Research Training Fellowships. These fellowships are open to men and women, citizens of the United States or Canada, who possess the Ph.D. degree or its equivalent in training and experience at the time of application, or give assurance that the Ph.D. degree will be received before February 15, 1946, and who, ordinarily, are not over 35 years of age. The closing date for receipt of applications for 1946–1947 on blanks to be secured from the Secretary to the Committee is February 1, 1946. When requesting application blanks, please be sure that age, academic qualifications, and proposed program of study are specifically indicated.

III. Grants in aid of research. The closing date for receipt of applications on forms provided by the Secretary to the Committee is January 15, 1946.

IV. Demobilization awards. (See the announcement in American Anthropologist, 47, 2 (April–June, 1945), p. 327.

On October 1, 1945, Demobilization Awards in anthropology were announced to John P. Gillin, for a study of the Ladino culture of eastern Guatemala; to Kenneth G. Orr, for research on the nature of ethnological resources in Oklahoma; to Harry Tschopik, Jr., for further graduate work leading to the doctorate in anthropology. Viola E. Garfield was given a Grant-in-aid for the completion of a study of Haida and Tlingit Indian economics.

GUGGENHEIM POST-SERVICE FELLOWSHIPS

The Trustees of the John Simon Guggenheim Memorial Foundation announce the establishment of Post-Service Fellowships in addition to their usual Fellowships. These are granted upon the same basis as the other Fellowships, to persons who have demonstrated unusual capacity for research and artistic creation. They will be available for the use of the recipients when they are discharged from service or within one year there-

after. No final date has been fixed for the receipt of applications for this class of Fellowship, but early application is urged to Henry Allen Moe, Secretary General, John Simon Guggenheim Memorial Foundation, 551 Fifth Avenue, New York 17, N. Y.

Among such Post-Service Fellows for 1945 were Dr. Roy Franklin Barton, for a continuation of his study of the Hudhud, a series of epics chanted by the Ifugaos of the Philippine Islands; and Dr. Donald Eugene McCown, for historical studies of the relationships of the civilizations of the Indus Valley and Mesopotamia during the third millennium.

PERSONALIA

Dr. Calvin S. Brown, well known for his work in geology and archeology, died on September 10 at the age of seventy-nine years. Dr. Brown was professor of Romance languages at the University of Mississippi.

Dr. Ralph Linton has been appointed Sterling Professor of Anthropology at Yale University, to take effect July 1, 1946.

Professor E. S. Pearson has offered to present to Cambridge University his father's very large collection of skulls and long bones. Temporary accommodation for the collection can be provided in the Museum of Archaeology and of Ethnology. Major J.C. Trevor, recently returned from war service and lecturing on physical anthropology at Cambridge University, will care for it.

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